PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VII. 1871–72. No. 84.

Eighty-Ninth Session.

Monday, 27th November 1871.

Sir ROBERT CHRISTISON, Bart., President, in the Chair.

The following Council were elected:—

President.
Sir ROBERT CHRISTISON, Bart., M.D., D.C.L.

Honorary Vice-President.
His Grace the DUKE of ARGYLL.

Vice-Presidents.

Professor Kelland.
The Hon. Lord Neaves.
Professor Sir William Thomson.
Principal Sir Alex. Grant, Bart.
Sir W. Stirling-Maxwell, Bart.
Professor W. J. Macquorn Rankine.

General Secretary—Dr John Hutton Balfour.

Secretaries to Ordinary Meetings.
Professor Tait.
Professor Turner.

Treasurer—David Smith, Esq.
Curator of Library and Museum—Dr Maclagan.

Councillors.

Professor Geikie.
Professor A. Crum Brown.
Rev. W. Lindsay Alexander.
Professor Fleeming Jenkin.
Prof. Wyville Thomson.
James Donaldson, Esq.
Dr Thomas R. Fraser.
Dr Arthur Gamgee.
Alexander Buchan, Esq.
Prof. A. Dickson.
D. Milne Home, Esq.
James Leslie, Esq., C.E.

Monday, 4th December 1871.

A Marble Bust of the late Sir Roderick I. Murchison, Bart., by Weekes, was presented.

Although the Bust was only placed in the Hall at this time, the offer of it to the Society was made by Sir Roderick I. Murchison in June 1871, in the following letter to the President :—

16 BELGRAVE SQUARE, 26*th June* 1871.

MY DEAR PROFESSOR,—As it is very improbable, indeed—nay, almost a certainty—that I shall not be able to attend the meeting of the British Association at Edinburgh this year, I wish to send, as my representative, a marble bust of myself, executed by Mr Henry Weekes, R.A., and which is on the point of completion.

I beg to be informed if the Council of the Royal Society of Edinburgh, over which you preside, will accept this bust as a donation from myself, in gratitude for the great honour they conferred on me many years ago, by enrolling my name in their distinguished list of honorary members; also in recollection of another great honour which they conferred on me, by granting to me the first Brisbane gold medal for my labours in Scottish geology. If you assent to this proposal, I will direct Mr Weekes to transmit the bust to the Secretary of your Royal Society, in the hope that you will place it in the same building as the busts of our other scientific countrymen whom you have thus honoured.

I have also written to David Milne Home on this point, and have assured him, at the same time, that I will do everything in my power to support the memorial to the Government to assist the Royal Society of Edinburgh in carrying out their meritorious researches, as signed by yourself.—An early reply will oblige, yours sincerely,

RODERICK I. MURCHISON.

To Professor CHRISTISON,
President, R.S. Edin.

Sir Robert Christison, Bart., the President, read the following Opening Address:—

At the commencement of this, the 89th session of the Royal Society of Edinburgh, I beg to congratulate you on the successful issue of that which has just come to an end. The number of our members has increased, in consequence both of a low proportion of deaths among us, and likewise of an increase of new members beyond the average; so that, from 326 at the same period last year, the Society has grown to 331 at the present time.

We may appeal with equal, and even more, satisfaction to the success of our late meetings; which, in the first place, were carried on a full month longer than usual before exhausting the list of communications approved by your Council as worthy of being read before you; and which, in the second place, attracted from first to last unusual attendance and interest, on the part both of ourselves and of our visitors, by reason of the variety and value of the inquiries communicated at them.

Nor, amidst these grounds of direct gratification on account of the proceedings of last year in the Royal Society itself, will it appear out of place that I further congratulate you on the great success which attended the late meeting in Edinburgh of The British Association for the Advancement of Science. Whether we consider who was the founder of this most prosperous institution—or that the Royal Society of Edinburgh and the Association were established very much for the same objects—or that our Fellows have taken an active part in its proceedings, wheresoever it may have held its meetings—or that our endeavours contributed greatly to bring it on the recent occasion to our city—or that many of us did much, or at least as much as we could, to receive our eminent guests with the cordiality due to their distinction in science—we are equally entitled to rejoice that, in respect of the number of remarkable men who were attracted hither, the excellence of the matter produced before the several sections, the interest of the excursions which the unrivalled opportunities in our neighbourhood enabled us to offer, the oft-expressed obligations of our guests for the reception they met from us and our fellow-citizens, and, I

may add, the eight days of glorious weather, upon which in Scotland much of the comfort of so great an assemblage depends—this forty-first meeting of the British Association proved in truth to be a great success.

Although the deaths in the Society have not been numerous during last year, we have nevertheless to lament the loss of several of the most distinguished among our Fellows, both ordinary and honorary. From the list of ordinary Fellows we have to strike out the names, in alphabetical order, of Dr William Anderson, Mr Charles Babbage, Mr Robert Chambers, Dr Robert Daun, Mr Alexander Keith Johnston, Dr Sheridan Muspratt, Mr Robert Russell, Sir William Scott, Dr Fraser Thomson, and Mr Moses Steven. Our honorary list no longer bears the names of Sir John Herschell, Sir William Haidinger, and Sir Roderick Impey Murchison.

Mr Robert Russell, an eminent practical and scientific agriculturist in the county of Fife, was led to connect himself with the Society by his taste for meteorological pursuits.

Sir William Scott, Baronet, of Ancrum, an enterprising country gentleman, a soldier in his youth, and afterwards for some time member of Parliament for his county, was well known for his attachment to scientific society, and for the regularity of his attendance at our meetings at a period when his avocations allowed him to reside occasionally in Edinburgh.

Dr Robert Daun, Deputy Inspector-General of Army Hospitals, also a frequent attender at one time of the meetings of the Society, died in June last at a very great age [86]. He served his country with distinction in the medical service of the army throughout nearly the whole of the most momentous period, and the most critical trials, in the military history of our country. He was highly esteemed publicly for his knowledge in all departments of his profession, and his powers of organisation in his own branch of service; and he was no less prized by his friends for his acquaintance with various branches of science and literature.

Dr Fraser Thomson, son of the Rev. Dr William Thomson of Perth, and nephew of the late eminent clergyman of Edinburgh, Dr Andrew Thomson, the first minister of St George's parish, graduated at the University of Edinburgh, where he had been a distinguished student of medicine. He settled as a medical practitioner in his native city, and for most of his life was much engrossed by the cares of an extensive practice in town and country. But, like many of his profession in our county towns, he made natural history his recreation for his short leisure hours, and applied himself eagerly to microscopical research in that department of science. In this he acquired great expertness and accuracy, and would easily have become an original inquirer, were it not that his fondness for such pursuits had not fame for its object, but simply relief from the cares and fatigues of professional life. He died, after a short illness, in the month of October, in his 65th year.

James Sheridan Muspratt, a native of Dublin, was trained in the science to which he dedicated his life, under two of the greatest chemists of their day in Europe—Graham and Liebig. At the age of twenty-three he published the results of investigations carried on as a student in Liebig's laboratory on the sulphites, showing their analogy with the carbonates. Returning to Giessen three years later, he resumed his inquiries into the sulphur acids, the fruit of which was an interesting paper on the Hyposulphites, and also on Sulpho-cyanic Ether. In the interval he did good service to practical chemistry in this country by making generally known in a translation Plattner's standard work on the Blowpipe; and in 1854 he published a "Dictionary of Chemistry," which has been of great use in diffusing a knowledge of chemistry among those engaged in the practical working of chemical problems. Mr Muspratt died in the 47th year of his age.

Mr Robert Chambers, long one of the most attached and working Fellows of the Royal Society, is one of the many instances, observed at all times in Scotland, of men raising themselves in a short time, by the sheer unaided gifts of native talent and indomitable perseverance, from an obscure position in society to a promi-

nent place in public estimation. Born, as we are told by one of his biographers, who evidently knew him and his history well, of parents respectable, but not fortunate in life, he had to struggle in his early years with difficulties. Nevertheless he was not prevented from reaping the inestimable advantages which in Edinburgh a parent of even moderate means could always command, for a son of promising parts, from an education at the High School.

Like other prolific writers, Mr Chambers began the career of authorship at a very early age. He must have been not above eighteen, when, having not long before chosen for his occupation in life that of bookseller, he determined to be publisher and author too, projecting and conducting a periodical called the "Kaleidoscope," to which he himself also contributed articles from his own pen. Soon afterwards he published "Illustrations of the Author of Waverley;" and in 1823, when only twenty years old, he added the work by which he has been longest and most familiarly known as a writer, his "Traditions of Edinburgh." Work upon work then followed in quick succession on all sorts of literary subjects, but chiefly historical and antiquarian—works which it would be out of place even to enumerate in so short a sketch as that to which this brief notice must be confined.

At last, in conjunction with his elder brother, Mr William Chambers, was begun in 1832 the now famous "Chambers' Edinburgh Journal,"—the first idea, and as such a great invention, of a weekly periodical devoted to short productions, original, as well as critical, on nearly all literary and also some scientific subjects, suited for the information, as well as for the purse, not alone of the educated classes ordinarily so called, but likewise for the educated in the humbler walks of life. This undertaking met soon with extraordinary success—in so much, indeed, that it became the parent of many others identical or similar in their aims, and not a few of them not less prosperous than that of the two brothers Chambers.

While adhering steadily to his literary tastes, and giving forth in various works the results of his literary labours, Mr R. Chambers' attention was turned to a totally different object of study, which in all probability he first followed as a diversion, or distraction

from the severity of professional toil. This was geology, which in the end captivated him, and first made him an active, energetic member of this Society. Cultivating his new pursuit with his inherent fervour unabated, he soon became an original inquirer in this fascinating branch of natural science. Besides making himself acquainted with the rock structure of many parts of his own country, he visited as a geologist Switzerland, Norway, Sweden, Iceland, the Faroe Islands, and parts of Canada and the United States. Few geological amateurs, engaged in a profession usually so engrossing as that of Robert Chambers, have acquired such intimate knowledge of geology. Many of us can recall the interest of his discussion of geological questions at our ordinary meetings; and his "Ancient Sea Margins" will long be known as one of the earliest, most exact, and most lively descriptions of that particular branch of his favourite study.

Mr Chambers was distinguished, alike in his public appearances, as in social intercourse, by a great fund of information on most diversified topics of interest in literature and science, by his caution and politeness in criticism, and by his courteous kindliness in every relation of life. In the last respect he will be long missed by a numerous circle of attached friends, many of whom were his fellow-members of the Royal Society of Edinburgh. In March 1871, after a tedious and enfeebling illness, borne with singular patience, he died in the 69th year of his age.

I turn next to another no less serious loss sustained during the past year by science and this Society in the death of Mr Alexander Keith Johnston. Mr Keith Johnston at first intended to join the medical profession; but, at an early age, he betook himself to the art of engraving, which again led him to the study of geography; and from that time geography became his ruling pursuit, and the object of his professional life.

In 1830, having had occasion, during a pedestrian trip in the Highlands, to remark the inaccuracy of the maps of Scotland, he published an improved collection in a Guide Book. At the same time, to facilitate the development of his geographical enterprises, he joined the firm of his two brothers, Sir William and Thomas Johnston, which had been established in this city some years

before for carrying on the business of engraving and printing, in which they have been long famous among the skilful engravers of Edinburgh. In his thirty-ninth year he attracted the regard of scientific geographers at large by the publication of his "National Atlas," and still more, five years later, by his "Atlas of Physical Geography," For the task he had thus set himself he had been thoroughly prepared by assiduous study of the best works in the various languages of Europe, by frequent visits to many European countries, and by acquaintance and personal intercourse with the greatest continental geographers and travellers. Not long afterwards Mr Keith Johnston brought out in succession a "Dictionary of Geography," a "Military Atlas" for Alison's "History of Europe," the "Royal Atlas of Modern Geography," and subsequently a variety of cheap atlases for the use of schools. By these productions he raised himself to a position in which he had no superior rival as a geographer in this country; and his merit in this respect received the stamp of the Royal Geographical Society of London in the last year of his life by the award of the Geographical Victoria Medal.

But Mr Johnston took also great interest in almost every branch of physical research, with many of which he had no mean acquaintance, and whose cultivation in this city he seized every opportunity to encourage and promote. Among other obligations to him, we are greatly indebted for the foundation of "The Meteorological Society" of Scotland,—an institution which, under the able direction of its present Secretary, promises important results, certain, indeed, to be realised if the Society receive due public support in the line of inquiry in which it has already been for some years successfully engaged. It is also known to me that the city and University are mainly indebted to him for the early foundation of the Chair of Geology, through the munificence of his friend the late Sir Roderick Murchison. At the direct instance of Mr Johnston, and through the weight which his genuine love of science commanded with many men of influence, Sir Roderick was induced to alter his intentions, from a "post-obit" foundation, to an immediate gift, of the Chair, in conjunction with a Royal Foundation and additional endowment.

In such proceedings as these Mr Johnston did good with no

ulterior view, and from no love of being what our neighbours across the channel aptly call a "grand faiseur." Hence we scarcely know how much we owe to him. His extensive acquaintance with the upper ranks of what it has become the custom to call the "citizen class" in Edinburgh, enabled him often quietly to direct public opinion in the nice exercise of scientific, literary, and professional patronage, when sound direction was greatly needed; and his acknowledged prudence, probity, impartiality, and knowledge of men, never failed to guide himself soundly in such conjunctures.

Throughout his whole life he was faithful and fruitful in his calling, and no less a sincere and active Christian. Seldom has there been a more affable, agreeable, and profitable companion in social life in all its phases.

Although far from being a young man at his death,—for he died in his 67th year,—we have to lament that he was struck down while in full possession of his powerful intellect, and enjoying shortly before a vigour which promised long continuance of his useful labours.

Wilhelm Ritter von Haidinger, one of our Honorary Fellows, was a favourite pupil of Mohs; who, during great part of the first half of this century, was celebrated as one of the foremost mineralogists of his day in Europe, and as the able Professor of Mineralogy in the University of Vienna. While yet a young man, William Haidinger possessed an extraordinary extent and accuracy of knowledge of minerals. On account of his talents as a descriptive mineralogist, he came to Edinburgh, about the year 1824, to arrange and catalogue the splendid mineralogical collection of a former curator of our Society, Mr Thomas Allan, banker in this city,—a collection unrivalled, for extent and careful costly selection, among the private mineralogical museums of Europe. In discharging this duty Mr Haidinger was enabled to establish several species as new to science; which he investigated and communicated to our meetings in conjunction with the late Edward Turner, the chemist, at the time lecturer here, and soon afterwards first Professor of Chemistry in University College, London. Haidinger took the descriptive, Turner the analytical, part of these inquiries; and, in both respects, their papers are models of

mineralogical investigation. I was at this time intimately acquainted with Haidinger, and could well appreciate his mineralogical facility and acuteness, his varied knowledge of natural history and physical science, and his remarkable command of languages,—so that, for example, in our own tongue, he could tell a jocular story, make a pun, and extemporise a clever couplet,—which I take to be about the severest of all tests of a man's familiarity with a foreign language.

No one who knew him at that time could fail to see that Haidinger would one day become a man of mark among the mineralogists of his own land, to which he returned soon after completing his labours in Mr Allan's museum. He then travelled for some time with Mr Allan's son, Robert, who died a few years ago a Fellow of this Society; and the main object of the travellers was the pursuit of mineralogy. Ere long Mohs died, and Haidinger succeeded him in his University Chair. His office put him naturally at th· head of all relative Government undertakings, which in their turn brought him promotion, till at length he filled the highest office in his profession, that of Director of the Mineralogical and Geological Survey of Austria. For his many scientific and practical services to his country he received from his sovereign the honour of knighthood a few years before his death, which took place last April in, as I understand, the 71st year of his age.

Coming nearer home, I have next to deal with the scientific life of another lost Honorary Fellow of the highest rank in Physical Philosophy, Sir RODERICK IMPEY MURCHISON, Baronet. But though very willing, and not altogether unable, to do justice to his remarkable labours in his science, I felt that I should be acting with injustice to his memory, and to the claims of a far superior biographer and eulogist, if I did not transfer from myself to Professor Geikie the pleasing task of recalling to our recollection the main points in the life and the work of his patron and friend. The following summary is accordingly the tribute which Professor Geikie has kindly enabled the Society to pay to the fame of Sir Roderick Murchison :—

"Among our recent losses there is none which we have more reason to deplore than his. The name of Sir Roderick Murchison

has been a household word in geology for nearly half a century, not in Britain only, but also over all the world. While we share in the wide regret at the injury which the general cause of science sustained by his removal, we add also the sadness which arises from the recollection of the relation which he bore to the progress of geology in Scotland, and from what he has recently done for the advancement of its study in the University of this city.

"Born in 1792 at Tavadale, in Ross-shire, he was educated for the military profession, and served during part of the Peninsular War. But on the arrival of peace in 1815, finding that the army no longer opened up the same prospect of activity for which he longed, he gave up his commission, married, and settled in England. The succeeding part of his life, prior to 1824, he used to speak of as his "Fox-hunting period," when he threw himself with all the ardour of his nature into the field sports of a country residence. Part of that period, however, he spent abroad, making, with his wife, tours in search of picture galleries and old art, and keeping an elaborate diary, with criticisms on the character of the fine arts in each tour or collection visited. It was by a kind of happy accident that his energies were at last directed into the channel of science,—the merit of which change was due partly to his wife's taste for natural history, and partly to the friendly counsel of Sir Humphrey Davy. He joined the Geological Society of London, and soon became one of its most enthusiastic members. From that time forward his love for geology, and his activity in its pursuit, never waned. He travelled over every part of Britain, and year after year he resorted to the Continent, traversing it in detail from the Alps to Scandinavia, and from the coasts of France to the far bounds of the Ural Mountains. As the result of these journeys, there came from his pen more than a hundred memoirs, besides two separate and classical works on 'The Silurian System,' and on 'Russia.'

"Sir Roderick was essentially a geologist, and he chose one special branch as his own domain. Perhaps no man ever had the same power,—which seemed sometimes almost an intuition,—of seizing the dominant features of the geographical and palæontological details of a district. With a keen eye to detect the characters as they rose before him, and a faculty of rapidly appreciating their

significance, he could, as it were, read off the geology of a country after a few traverses only, when most men would have been puzzling over their first section. This was the secret of his broad generalisations regarding the geological structure of a large part of Europe,—generalisations which, though of course requiring to be corrected and modified by subsequent more detailed investigations, still remain true in the main, and still astound by their marvellous grasp and suggestiveness. The leading idea of his scientific life was to establish the order of succession among rocks, and through that order to show the successive stages in the history of life on our globe. With the more speculative parts of geology he meddled little; nor did he ever travel outside the bounds of his own science. He early recognised the limits within which his powers could find the fullest and most free development, and he was seldom found making even a short excursion beyond them.

"The special part of his work on which his chief title to fame rests is undoubtedly his establishment of 'The Silurian System.' Before his time, the early chapters of the history of life on our globe had been but dimly deciphered. William Smith had thrown a new flood of light upon that history by showing the order of succession among the secondary rocks of England, and had done more than any other man to dispel the prejudices with which the doctrines of Werner seemed naturally to fill the mind. But the rocks older than secondary, to which Werner had given the name of 'Transition,' remained still in deep Wernerian darkness. Sir Roderick Murchison saw that it might be possible to bring order and light out of these rocks, even as had been done with those of more recent origin; and that a double interest would attach to them if, as he supposed, they should reveal to us the first beginnings of life upon our globe. Choosing a part of the broken land of England where the rocks are well exposed, he set himself to unravel their order of succession. Patiently year after year he laboured at his self-appointed task, communicating his results sometimes in writing to his friends, sometimes in the form of a short paper to the Geological Society of London, until at last, in 1838, he gathered up the whole into his great work, 'The Silurian System.' In that book the early chapters of the history of life on the earth were first unfolded, and a system of classification was

chosen with such skill that it has been found applicable, with minor modifications, even in the most distant quarters of the globe.

"Round this early work all his after-labours seemed to range themselves by a natural sequence. His choice had led him into the most ancient fossiliferous rocks, and to that first love he remained true. Whether in the glades of Shropshire, or the glens of his own Highlands, among the fjelds and fjords of Norway, or in the wilds of the Urals, it was with the Palæozoic formations that he mainly busied himself. They were to him a kind of patrimony which had claims on his constant supervision. With his friend Sedgwick he unravelled the structure of the middle Palæozoic rocks of Devonshire, and with Keyserling and De Verneuil he showed the true relations of the upper Palæozoic rocks of Russia. The Silurian, Devonian, and Permian systems, representing each a vast cycle in the history of our earth as a habitable globe, received in this way from him their first clear elucidation, and the very names by which they are now universally known.

"But if we seek to measure the influence which Sir Roderick Murchison exercised on the progress of the science of the time merely by the original work which he himself accomplished, we should fail duly to appreciate the measure and the power of that influence, and the extent of the loss which his death has caused. Fortunate in the possession of wealth and high social position, he was enabled to act as a constant friend and guardian to the cause of science. He moved about as one of the representative scientific men of his day. To no man more than to him do we owe the public recognition of the claims of scientific culture in this country. For he not only stood out as the acknowledged chief in his own domain, but had also the faculty of gathering round him men of all sciences, among whom his kindliness of nature, his courteous dignity of manners, his tact and knowledge of the world, and his wide range of social connections, marked him out as spokesman and leader. Nowhere were these features of his character and influence more conspicuous than in his conduct of the affairs of the Geographical Society, of which he was for many years the very life and soul, and which owes in large measure to him the stimulus it has given to geographical science.

"Here in his own native country, and more especially here in

Edinburgh, we have peculiar cause to mourn the loss of such a man. Though his residence from boyhood had been chiefly in London, he never to the last relinquished his enthusiastic regard for the land of his birth. He never lost an opportunity of boasting that he was a Scot. During the last ten years of his life he made frequent and protracted tours in the Highlands; and, in unravelling their complicated geological structure, he accomplished one of the most brilliant generalisations of his long and illustrious scientific career. There is something touching in the reflection that, after having travelled and toiled all over Europe, gaining the highest position and rewards which a scientific man can attain, he should at last, ripe in years and in honours, have come back to his own Highlands, and there completed his life-work by bringing into order the chaos of the primary rocks, and laying such an impress on Scottish geology as had never been laid before by any single observer. For these and other researches he received from this Society the first Brisbane Medal—an honour conferred on him at the Aberdeen meeting of the British Association, and of which he often spoke as one that gave him the deepest gratification. He used to boast, too, of being an honorary Fellow of this Society, and to quote a remark made to him by the late Robert Brown, that his election into the list of our honorary Fellows was one of the highest marks of distinction he could receive. His kindly interest in our prosperity was often expressed; and we have a token of it in the presentation to us of his bust by Weekes, which this evening is formally delivered to the Society.

"Of the closing acts of his life, there is one which cannot be mentioned without peculiar pride—the institution of a Chair of Geology and Mineralogy in the University of Edinburgh. He intended to found this Chair by bequest; but on the retirement of Dr Allman from the Chair of Natural History, he determined to do in his lifetime what would otherwise have been accomplished not till after his death. He gave to the University a sum of £6000; and the Crown having consented to add an annual grant of £200, the Chair was founded in the spring of the present year. Sir Roderick has not lived to witness the first beginnings of the tuition which he had started. But long after the memory of his personal character shall fade, men will remember the work which

he did; they will recognise the impetus his researches have given to geology all over the world; and let us hope also they will see in the Chair he has founded the starting-point of a new and active school of Scottish geology."

I have left to the last in this biographical sketch of our lately deceased Fellows two of the most eminent men of British science in their day—Herschel and Babbage. For as I could not pretend to do justice to the lives of men whose pursuits, in the highest range of physical science, were so far removed from my own, I think it right to keep quite apart the following eulogium, the preparation of which my university colleague, Professor Tait, has kindly allowed me to impose on him, and which I will give in his own words:—

"Of Sir John F. W. Herschel and Charles Babbage, who may be fitly mentioned together, it is not necessary that much should be said, as their contributions to science cannot fail to be set forth at length in the Proceedings of other Societies, with which they were more connected than with our own. Intimate friends during their undergraduate career at Cambridge, they joined us as ordinary Fellows shortly after taking their degrees, and when they were just commencing, along with the late Dean Peacock, what all must consider, in spite of their other grand contributions to science, the greatest work of their lives—the restoration of mathematical science in Britain. It is impossible even now to overestimate the value of this service. Few know to what a state of ignorance we had fallen at the time when Lagrange, Laplace, Fourier, Cauchy, Poisson, and Gauss, and many others abroad, were advancing with breathless rapidity in the track, neglected by us, of James Bernoulli and Euler. Partly from a mistaken notion that they were honouring Newton by adhering to his published methods, partly owing to the British dislike to men and things foreign, which at this time was pushed, perhaps not unnaturally, to extreme lengths in all matters, and partly in consequence of our long state of war with France, our mathematicians had never even learned those unpublished methods by which Newton made his discoveries, which, as soon as they were to some extent divined

abroad, were at once estimated at their true value, and pursued with zeal and genius.*

"Little by little, first by translating Lacroix's elementary treatise on the differential and integral calculus, and by thus introducing, in face of determined opposition, the notation of differential coefficients into Cambridge, so as for the first time to enable her mathematicians to understand a foreign treatise; secondly, by publishing an excellent collection of examples; and thirdly, by their separate original treatises on different special parts of analysis, they put this country on a level with France and Germany, so far at least as opportunities of progress are concerned. It is to them mainly that we owe, not merely our modern British school of mathematicians, which is now certainly second to none in the world, but even the very possibility of the existence in this country of such great departed masters as Boole and Hamilton.

"Herschel's 'Treatise on Finite Differences,' which appeared as a supplement to the translation of Lacroix, is one of the most charming mathematical works ever written, everywhere showing

* Professor Tait has urged me to make known a reminiscence of my youth that at the time here referred to there were in Edinburgh, and in this Society, no fewer than three mathematical amateurs, who, though they never made themselves publicly felt as such, in some measure saved this corner of the land from the censure dealt in the text. These were Sir William Miller, Baronet, of Glenlee, better known as Lord Glenlee of the Scottish bench; William Archibald Cadell, of the family of Cadell of Grange, who finished his earthly career but a few years ago; and my own father, Professor of Latin in our University. Lord Glenlee, a man of very retiring habits and disposition, was usually called the first amateur mathematician in Scotland. Mr Cadell, also a man of great reserve and shyness, nevertheless, in order to carry out his admiration of the modern continental mathematics, contrived to obtain, during the very hottest of our struggles with France, from that generally unyielding potentate, the First Napoleon, permission, through the influence of one of the great mathematicians of Paris, to repair to the French capital, to dwell there for seven years, and to return unhindered to Scotland, at a period when no other Briton was known to have put his foot on French soil without being made a *detenu*. My father, during the last ten years of his life, which ended in 1820, betook himself, as his idea of relaxation from routine professional life, to the differential calculus, and to Newton, Bernoulli, Euler, Lagrange, Laplace, Lacroix, &c., whose works were always at hand when not in his hands. As he made a vigorous attempt to indoctrinate me at a very early age in his favourite pursuits, I know well what these were, and what he knew of the kindred spirits Glenlee and Cadell.

power and originality, as well as elegance. In all these respects it far surpasses his subsequent mathematical writings, excellent as are many of them; for instance his celebrated treatises on 'Light' and on 'Sound' in the 'Encyclopædia Metropolitana.' The appendix to Lacroix which was written by Babbage, was devoted to the 'calculus of functions,' a strangely weird branch of analysis, which remains even now much as Babbage left it. That in this direction there is a splendid field open for the inquirer, is evident to any one who consults Babbage's papers on it; and it is wonderful that it has not been greatly developed of late years, when so many mathematicians, especially at home, have been found to apply themselves almost exclusively to those branches of the science which seem the least likely ever to have useful applications.

"In their after-life the careers of these great workers and thinkers led them widely apart. Herschel devoted himself mainly to astronomy, but also to chemistry, photography, and occasionally to mathematics. His astronomical work is all of the very highest class, whether it consisted in his seclusion, for several of the best years of his life, at the Cape of Good Hope in the close observation of the stars and nebulæ of the Southern Hemisphere; or in first writing, and then, as edition after edition was called for, extending and improving his splendid semi-popular work, the 'Outlines of Astronomy,' which none, even of men of science, can read without deriving from it at once pleasure and profit.

"Babbage, on the other hand, applied himself mainly to machinery and manufactures. His so-called 'Ninth Bridgewater Treatise' was pre-eminent even among the best of that singular series; his 'Economy of Machines and Manufactures' is still a wonderfully suggestive work; and his 'Mechanical Notation' supplies us with an insight into the kinematics of all possible combinations of machinery, which none can have any conception of without making it a special subject of study. He was led to its invention by his celebrated attempts to achieve the construction of a difference-engine, and even of an analytical engine—machines totally unintelligible, in their conception, to the majority even of those who are capable of understanding the nature of the work for which they were designed. Enough was constructed, though it was a very small part, of the first of these engines to show not only that

the device was completely successful, but also to exhibit the extraordinary talent of the inventor in such a light as to convince scientific men that in his hands the astounding problem of constructing the second was capable of solution. A paltry economy of the Treasury prevented the completion of the first engine, and made it obvious to Babbage that there was no hope of assistance from Government to construct the second. Yet it has been allowed by the best authorities that the money spent on the finished portion of the difference-engine was far more than repaid to the country by the extraordinary improvement in tools of every kind, which was required for the new engine, and was at once supplied by the fertile, inventive brain of Babbage as the work proceeded.

"No one can read the obviously true story of this miserable affair, as it appears in the strange autobiography of Babbage—his 'Passages from the Life of a Philosopher'—without a blush for the short-sightedness of British rulers. Had Babbage been a Frenchman or Russian, had he even belonged to the then poor kingdom of Prussia, do we not all feel assured that these grand conceptions of his would long ere now have been realised as powerful agents in the working world, instead of lying dormant, in mouldering, worm-eaten plans and sections.

"Strange the contrast between the careers of these early friends! They began, indeed, by a grand joint success, for which alone their memory will always be justly cherished. But while the one, encouraged, yet never unduly elated, by success, steadily at work, though not of late years brilliantly, ended a long and happy life, every day of which had added its share to his scientific services; the other, enraged by the petty persecutions of men unable to understand scientific merit, or even its mere pecuniary value, spending lavishly from his private fortune to be enabled to leave to some possibly enlightened posterity a complete record of the working details for the construction of his splendid inventions, was never understood by his countrymen.

"But so it has ever been in this country. Herschel's father was a German; so of course we could appreciate him. Babbage was an Englishman; the only person who took the trouble to understand his invention was a foreigner, the skilful mathematician Menabrea, ex-minister of Victor Emmanuel."

Observations on the Fresh Waters of Scotland.

Looking around me for some general theme suitable for the subject of this introductory address, I became oppressed with the persuasion, that no such subject, worthy of your acceptance, had been left unexhausted by the able men who have lately had to treat of scientific topics of a general nature in circumstances akin to my own on the present occasion. I therefore thought I might trust to your indulgence, and substitute for a general address a notice of some inquiries, which have been carried on from time to time during my late occasional autumn holidays, and which promise results of some interest, illustrating the hydrography of the fresh waters of Scotland. These inquiries have in several respects been pushed not so far as to satisfy me completely. But as I may not be able to carry them through according to my present design, and I hope that others may be led to interest themselves in also pursuing them, I beg to submit the results to the Society, such as they are.

The topics I propose now to bring forward,—which are rather diverse in nature, yet not altogether unconnected with one another, —are three in number,—*First*, The composition of the water of certain lakes and their leading streams in Scotland, and the changes their waters undergo in the streams which the lakes feed; *Secondly*, The temperature of these lakes at various depths; and, *Thirdly*, The action of their waters upon lead.

I shall commence by recalling shortly the geological structure of our country, by which in a great measure the nature of its waters is regulated.

In the primitive formations which constitute the "Scottish Highlands" of ordinary speech,—for in correct language many parts of the so-called "Lowlands" are as well entitled to the other name,—we find that the mountain summits are either pointed or rounded, but seldom table-topped; that their spurs are commonly rather sharply ridged; that their surface abounds in precipices, crags, loose blocks, rocks, and stones; and that the valleys between them, except in the course of our largest rivers, are narrow, gravelly, or rocky, thinly covered with vegetative soil, and consequently little fit for plough cultivation. Not infrequently, however, the spurs or buttresses, instead of being ridgy, are broad and flat,

smoothly covered with fine heather, the favourite breeding-place for grouse, and tolerably dry, except where small patches of peaty bog show themselves here and there. This structure is often well exemplified among the mountains of Glen-Shee. Again, when the spurs of a mountain are ridgy, the ridges are sometimes separated from one another by an upland valley, often very grassy, especially towards its head or "corrie," but likewise apt in many places to be boggy, and there abounding in peat, and in denuding cuts which expose the peat to atmospheric influences. Good examples of such upland valleys are to be seen on the Cobbler, and on its higher northern neighbour Ben-Arnen, where they face Arrochar eastward, and also on Ben-Lomond northward from its peak. Exposed peat constitutes on the whole no great proportion of the surface of most mountains in the Highlands.

It follows from this structure, that in most districts of the Highlands rain and melted snow find little to dissolve in descending the mountain sides; and their steepness causes the streams to tarry a very short time in their descent, and to drain off quickly the excess of water in flood-time. All these circumstances combine to render the streams and lakes of the Highlands uncommonly pure in dry weather, and not materially less so even in heavy floods. Among the granite ranges, such as in the Goat-Fell district of Arran, the streams, such as the Rosa and Sannox, are beautifully clear and colourless in the highest floods. The temporary water-falls which then streak the mountain slopes, present to the eye the purest whiteness; and on filling a glass tumbler from a stream, the water, after the instant subsidence of a few coarse particles of granite sand, is seen to be perfectly transparent and free from colour. In the mica-slate districts of the near Grampians the streams are equally pure in dry weather. But after rains they are visibly brownish, yet so slightly that in a common water-bottle on a dinner-table the colour may readily escape notice.

During last autumn I had frequent opportunities of examining, in various circumstances, the water of one of these mica-slate streamlets, which is used for supplying a villa near Loch-Goil-head. The stream descends the steep eastern slope of "The Cruach," a hill which land-locks the upper part of Loch Goil on its west shore at a point about a mile and a half from the Head. Although only

2000 feet high, "The Cruach" presents an imposing, rugged, conical sky-line to one entering Loch Goil from Loch Long. The east face, precipitous at the summit, is entirely grassy lower down, unless where broken by other precipices, out-cropping rocks, or stream-courses, also always rocky. There is little peat to be seen anywhere, and no agriculture. From various trials around Loch Goil and Loch Lomond I am satisfied that this streamlet is a fair type, both in its ordinary state and in its occasional variations, of most of the streams which tumble into these sheets of water from the mica-slate mountains around them.

When I examined this water in the end of September, after ten days of perfectly dry weather, following a heavy twelve-hours' rain two days earlier, it was beautifully clear and sparkling. In the first place, it was entirely free from colour. The absence of colour was tested conveniently and delicately by means of a glass tube 16 inches long and six-tenths of an inch in diameter, which is nearly filled with the water to be examined, and is held over, but not touching, a sheet of white paper in a bright light. For security, a very fine colourless spring water was always kept at hand for comparison in another tube. The slightest coloration is thus seen by looking perpendicularly down the tube. Or it may be equally recognised by looking at the surface of the water obliquely through the upper part of the tube from a distance of 18 inches or 2 feet; for the colour is thrown up by the paper, and concentrated, as it were, on the surface of the water, though the long subjacent column, as seen through the glass, appears colourless. Very few waters, except that of springs, withstand altogether this test of the presence of colour.* Mr Dewar has suggested that it admits of being made a water-chromometer, by employing for comparison,—distilled water being used for fixing the zero point,—a solution of some invariable strength of a permanent per-oxide salt of iron, such as the acetate, and diluting the solution to uniformity of depth of colour with the water to be compared. The amount of dilution would denote the degree of coloration relatively to a fixed standard.

In the second place, this water contained a very small propor-

* This method, devised for the occasion, I have since found to be a mere variety, but more convenient, of one proposed some years ago by Dr Letheby, and adopted by the late Professor Miller.

tion of saline matter. In by far the greater number of streams and lakes in Scotland, whether Highland or Lowland, the salts met with are the same, viz., carbonates and sulphates of the three bases, lime, magnesia, and soda, and the chloride of their metalloids, calcium, magnesium, and sodium. Of these the chlorides are usually most abundant, the sulphates least so; and of the bases, lime is commonly predominant, magnesia the contrary. But frequently in the Highland streams the proportion of all is so small that most of the ordinary liquid tests scarcely affect them. In the water now under consideration, for example, magnesia, among the bases, was not indicated by the alkaline phosphate of ammonia; nor was sulphuric acid, among the acids, by nitrate of baryta; even lime was doubtfully indicated by oxalate of ammonia; chlorine, too, was scarcely indicated by nitrate of silver in a small test-glass, and required a quantity amounting to six or seven ounces to yield an undoubted faint mist; and permanganate of potash did not denote organic matter except faintly. Acetate of lead, however, by acting on both combined carbonic acid and organic matter, showed a haze even in a small quantity of the water; and so did tincture of potash-soap, by virtue of the decomposing influence on it of earthy carbonates and free carbonic acid together.

After frequent trials I am inclined to think, that for practical purposes, when organic matter does not require to be taken into account, we seldom need any other test for ascertaining the relative purity and usefulness of these waters than the late Professor Clark's soap-test. In the present instance this denoted in several trials only 1·04 degrees of hardness, which is equivalent to that much of carbonate of lime in an imperial gallon of 70,000 grains of water. From frequent observation of the effects of this and other liquid tests, I feel assured that the total solid contents could not have been more than a 25,000th of the water, and was probably nearer a 30,000th.

In the third place, this composition, viz., little saline and extremely little organic matter, would lead to the expectation that the water will corrode lead. And so it does, but not powerfully. A thin plate of lead, with 4½ square inches of surface, weighing 437 rains, was suspended by a lead rod in this water. In twenty-eight

days it lost only 0·42 grain in weight, and crystals of carbonate of lead were deposited scantily. In circumstances exactly the same, distilled water will form carbonate of lead in abundance, and the loss of lead is 3·4 grains, or eight times as much.

In times of flood the condition of the water in such streamlets necessarily undergoes change. But the difference is not so great as might naturally be expected. In the night of 19th September last and subsequent morning rain fell steadily at Loch Goil, and heavily for twelve hours; and, consequently, in the forenoon of the 20th the streamlet described above was considerably flooded. The water, seen in bulk, was somewhat brownish; it was even faintly brownish in a dining-room water-bottle; and in a 16-inch glass tube it appeared yellowish. Nevertheless, it looked well enough in a glass tumbler, and it was not in the slightest degree turbid. Its purity, apart from its colour, was very great. No liquid test for inorganic salts but one,—not oxalate of ammonia, not nitrate of silver, not even acetate of lead, had any visible effect. The soap-test alone exerted any manifest action; and this indicated only 0·8 degrees of hardness, which is equivalent to little more than an 80,000th of carbonate of lime in the water. In correspondence with this condition, lead underwent rapid corrosion in it. A plate, an inch and a half square, lost in twenty-eight days 3·09 grains in weight, or about $\frac{13}{14}$ths of the loss in distilled water in the same time; and crystals of carbonate of lead were formed in abundance.

I examined the same stream on a previous occasion after a furious tempest and rain-flood on the 24th August last. Much rain had fallen at Loch Goil previously for several days. But on the 24th it fell in torrents, and for half-an-hour that forenoon like a tropical deluge. During this period a great extent of grassy turf was torn off in the upper part of the stream, probably by a waterspout. In a few minutes the streamlet, already in high flood, became a muddy tumultuous torrent in which no man could have stood or lived; swiftly its muddy waters spread out over the salt water of Loch Goil; and then meeting similar floods first at its own side, and afterwards from the opposite shore, the united muddy torrents covered the whole upper reach of the loch in less than half-an-hour to the extent of two miles in length, and three-quarters

of a mile in average breadth. A rainy day followed, and then four days of uninterrupted dry weather, during which the stream returned nearly to the same state in volume and appearance as after the moderate flood already described. There was this difference, however, even in its composition; nitrate of silver feebly indicated chlorides, and acetate of lead also feebly indicated carbonates. The difference wss probably owing to a material difference in the direction and force of the wind. On the former occasion the wind blew from the north-east, with no great violence, over about 90 miles of land; but on the latter occasion it blew with fury from west to south-west over Loch Fyne at distances varying from 18 to 15 miles only. In the latter case sea-spray must have been swept up into the air and carried far by the storm. In the former less would be raised into the atmosphere, and much would be deposited again in passing over 90 miles of land. In 1845 I found chlorides distinctly indicated by a white cloudiness, when nitrate of silver was added to rain-water collected on the top of Goat-Fell in Arran, towards the close of a violent four days' south-westerly gale, attended with frequent heavy rain, the sea in the direction of the wind being 12 miles distant, and 2800 feet below.

The facts now stated, which I have often corroborated by less minute observation of other streams in the mica-slate district of Loch Long, Loch Goil, and Loch Lomond, will convey some idea of the constitution of these waters in three conditions, viz., after high floods, moderate floods, and dry weather. To complete the series, it is an object of interest to add their condition after very prolonged drought. In that case the streamlets, except those fed by small upland "tarns," will come at last to convey only the water proceeding from springs; and many not so supplied will dry up altogether. For the composition of those which continue to run we may look to the springs themselves which feed them, because in their then very low state, running chiefly over rocks and stones, their waters will contract little additional impregnation in their course downwards. I have examined several springs in the mica-slate district under consideration. They have generally presented rather more saline constituents than the streams in their ordinary state, and invariably no colour appreciable by any of the ocular

tests I have used as described above. Sometimes their salts are scanty; but always they are quite colourless. Their solids appear to vary from a 16,000th to a 21,000th; and chlorides and lime-salts are, for the most part, indicated by their proper liquid tests rather more distinctly than in the general run of stream- waters in their ordinary state of fulness. Several small springs high on the hill slopes have yielded these results. Similar in that respect is a copious spring in Glen Beg, more familiarly known by the name of Hell's Glen, about three miles from Loch-Goil-head in the narrow pass to St Catherine's on Loch Fyne. This spring, which gushes in force near the highway and close to the valley stream, is at all times beautifully limpid, and seems to be little affected in volume by droughts or floods. Its temperature is 44° when the air is 64° and more, though its site is not much over 300 feet above the sea-level. Its water is perfectly colourless, but contains rather more chlorides and earthy salts than the waters of the streams in their ordinary condition. Another more remarkable spring of great volume issues from the south flank of the Cobbler, about 1500 feet perpendicular above the bottom of Glen Croe, and leaping from rock to rock, joins the Croe about half-way up the glen. In the very dry season of 1870, its course was the only one which showed any water among the many which score the steep slope of the mountain where it overlooks the glen from the north. I found the water last autumn, after ten days of complete drought, to be perfectly colourless, and to be so free from saline matter as to be barely affected even by the delicate liquid tests for chlorine and for lime.

As the various streams now described are the feeders of the fresh-water lakes, which abound in the mica-slate districts, the composition of the water of the lakes must be the same with that of the average water of the streams. The small upland "tarns" are peaty, owing to the peat which paves and surrounds them. But the great low-lying lakes present very little solid matter of any kind in their waters; their scanty salts consist of chlorides, carbonates, and sulphates, the bases being lime, soda, and magnesia; and the organic colouring matter is so small as to be discoverable by delicate tests only. In all instances, however, our purest lake

waters in a mica-slate country are slightly — very slightly coloured.

The water of Loch Katrine is a well-known and characteristic example. Some years before the proposal was first entertained to use it for supplying Glasgow, I found it to contain only a 40,000th of solids. When compared with a fine spring water, however, it now presents in a 16-inch glass tube an appreciable, yet very faint, yellowness. In hardness it indicates only 0·65 by the soap-test, or the equivalent of a 108,000th of carbonate of lime. In correspondence with this great purity it acts powerfully on lead. In three weeks, a lead plate one inch and a half square, lost 2·53 grains in weight, which is exactly the loss sustained in distilled water in the same time; and crystals of carbonate of lead were formed in profusion.

The water of Loch Lomond is a less familiar instance of the same kind.

Loch Lomond is twenty miles long, and at its southern or outlet end, rather more than four miles and a half wide. Its average elevation is only 22 feet above high-water mark. Eight miles north of its outlet it suddenly contracts at Ross Point to rather less than a mile across; and the northern division of twelve miles in length varies in breadth between a mile and only a fourth so much. The lower wide division of the loch, at a short distance from the shore, varies in depth on the whole from 8 to 12 fathoms; and these soundings continue till near Point Ross, where there is a rapid increase to 32 fathoms. This continues to be the average in the middle of the lake, till at the next contraction in its width, opposite Rowardennan Point, where it singularly shallows at once to 9, 8, and 7 fathoms. A mile further up, after another swell, it quickly deepens at a new contraction at Rhuda Mor (the Great Point) to 65 fathoms; and for five miles further north the soundings first steadily deepen by degrees to 105 fathoms, and then shelve to 80 opposite Inversnaid; above which point the lake becomes both much narrower and greatly less deep (Admiralty Map). My observations on its waters were made near Tarbet, which faces the middle of the very deep five-mile reach, where the soundings in mid-channel are never under 85, and at one place, opposite Culness farm-house, attain the extreme depth of 100 and

even 105 fathoms,—the width there being barely three-fourths of a mile.

The surface water over these great depths is of remarkable purity. Its saline matter is very scanty, and the colouring organic matter equally so. Still it has a faint yellowish colour. On September 21st, the second day after heavy rain, incessant for twelve hours, a white porcelain basin, 4 inches in diameter, disappeared in 18 feet of water; on 11th October, after many days of alternate rain and drought, in 15 feet; and on 18th November, after four days of dry weather, in 14 feet, but in feeble sunshine.* After long drought there is little doubt that the colour would be less, for it will be seen subsequently, that as the streams pour in fresh supplies of water, there is reason to suppose that these penetrate little before they run off, and consequently the coloured flood water from the streams will colour for some time the superficial waters of the lake.

On 18th November, the water taken from the surface of Loch

* This is a good method of ascertaining the relative colour of waters if it be employed with due precautions. The trial should be made in sunshine—when the sheet of water is quite calm—between 9 A.M. and 3 P.M., so that the sun's rays may not fall too obliquely on the water, and with the back to the sun, and, best of all, on the shady side of a boat. If all these conditions be reversed, vision will penetrate scarcely half so deep as when they are all observed. In my recent trials I have not found a white object visible at a greater depth than 21 feet, viz., on Loch Lomond on the 6th May. But, from observations made many years ago, I am satisfied that, after long dry weather, some river waters will allow such an object as a white porcelain basin to be seen at a much greater depth, with due attention to the conditions now mentioned. Having a recollection of seeing it stated long ago, that the water of the Lake of Geneva was so clear, that objects could be distinguished in it at a very great depth, I applied to Dr Coindet of Geneva for precise information, for which he referred me to Professor Forel of Lausanne. To Professor Forel's kindness I am indebted for the following interesting facts:—In the spring of 1869, using a white-painted sheet of iron, 15 inches by 12, he found that the utmost depth at which it could be seen was 13 metres, or 44 feet. The transparency is much affected by locality, and very much too by season. In winter and spring it is greatest, in summer and autumn least. In the Bay of Morges, objects may be seen distinctly at the bottom in winter at a depth from 13½ to 20 feet, while in summer they are barely visible through 7 feet. This difference is greatest near the shore, at the bottom of bays, and near villages or towns. It is least around promontories, far from land, and at a distance from human habitations. In autumn the change from obscurity to transparency usually takes place early in October, and is completed in three days; in summer, the reverse change takes place

Lomond over a depth of 102 fathoms, or 612 feet, presented in a 16-inch tube as exactly as possible the same degree of faint yellowish hue as the water of Loch Katrine. Evaporated to dryness, it left a pale, greyish film, amounting to a 33,000th of the water. It had only 0·70 degrees of hardness by Clark's soap-test. Of the other liquid reagents, acetate of lead alone caused at once a slight haze; oxalate of ammonia and nitrate of silver had at first no effect, but in time caused an extremely faint haziness; nitrate of baryta, and ammoniacal phosphate of soda had no effect at all. When the water was much concentrated, however, sulphates, carbonates, and chlorides, as well as the bases, lime, soda, and magnesia, were clearly indicated by their ordinary tests, exactly as in the springs and streams of the adjacent country.

I examined also the water taken at the same place from the bottom at the depth of 102 fathoms. This differed in some respects from the surface water directly above it. It contained the same salts. But nitrate of silver indicated rather less chlorides; acetate of lead more carbonates; the soap-test denoted a trifling additional hardness, namely 0·74 degrees, and the total solids amounted to a 28,000th instead of a 33,000th. Farther,

about the beginning of May, and is more gradual. By filtering a large quantity of turbid water, he found the obscuring cause to be a collection of amorphous dust, living and dead diatoms, vegetable debris, a few living infusoria and crustaceans, and debris of insect larvæ and microscopic crustacea. They naturally collect slowly in the summer; but the first cold of approaching winter sends them quickly down with the water as it cools.

In the case of Loch Lomond, these inquiries of Professor Forel would lead one to expect little influence from organic or inorganic dust in obscuring water where it is so deep as at the places chosen for my observations. Accordingly, the surface water was remarkably free from turbidity, or deposit on standing at rest. But the yellowish colour, faint though it be, constitutes a no less powerful obstruction to the penetration of light. The depth of colour, and consequently the transparency, vary at different periods, not so much with the seasons as with the times of floods. In advanced summer and in autumn, the floods increase the colour decidedly, and lessen for a time transparency. But my single observation on 6th May, when I found the transparency greatest of all a few days after heavy north-east rain, raises a question whether floods have the same effect in spring or the end of winter. A probable reason for the contrary may be, that the soluble matters of the peat-fields and stream-courses, developed by heat, growth, and atmospheric action in summer and autumn, are much exhausted by the frequent winter floods before the arrival of the floods of spring.

although the colour is the same at the bottom as at the surface, and very slight, it is distinctly deeper in shade when seen in a 16-inch tube; and the film left on evaporation, instead of being light grey, is of a rather deep yellowish-brown tint.

[*May* 16*th*, 1872.—As supplementary to these observations, I may here add the following, which I had an opportunity of making on the 10th of last month:—During the five winter months intermediate between my previous visit in November, the winter had been unusually open. Until the middle of March, indeed, there had been very little frost, and no severe cold. During the latter half of March frosty northerly winds prevailed, but without any very great fall of the thermometer. In the last days of March and first three days of April, snow fell frequently, covering the Highland mountains to their bases. Ben Lomond and the adjacent Arrochar mountains shared in the change. On 4th April the wind veered to west and south-west; bright sunshine and warmth soon dissolved most of the snow, and this weather continued, with scarcely any rain, till after my visit. The ground around Loch Lomond was consequently dry, the hill streams very low, and the streamlets dried up, or nearly so.

The surface water corresponded with these antecedent circumstances. Frequent winter floods had swept from the mountains most of the soluble matter from their beds; and for some days the streams, reduced to rills, would have little remaining to remove from their stony channels. Hence the surface water was of great purity. A white porcelain basin, two inches in diameter, was visible at the depth of 16 feet, although a light breeze rippled the surface. In a 16-inch tube the yellowish colour was extremely faint. The solid contents amounted to only a 32,000th of the water, and lost a fourth by incineration.* Nitrate of silver occasioned in the water only the faintest haze, and oxalate of ammonia did not visibly affect it. The soap-test indicated 0·49 of hardness, which is equivalent to a 145,000th of carbonate of lime. In accordance with its purity this water acted powerfully on lead. Action commenced at once, loose crystals of carbonate of lead were formed

* 26,250 grains left 0·83 at 300° F., and 0·62 after incineration.

in abundance, and in twenty-three days a plate an inch and a half square lost 1·11 grain in weight.

The bottom water, taken where the depth was 594 feet, differed materially in these characters. The cistern brought up some finely comminuted peat-like matter, in which the microscope detected a profusion of various diatoms, and two species of active microcosmic animals. The colour of the water was deeper than that of the surface, and became the same not till the addition of half its volume of colourless distilled water. Nitrate of silver produced an immediate scanty precipitate, oxalate of ammonia scarcely any effect. The soap-test indicated 1·015 of hardness, which is the equivalent of a 69,000th of carbonate of lime. The solids amounted to a 16,000th of the water, and lost a third by incineration.* When the water was evaporated to a tenth of its volume, nitrate of silver indicated chlorides in abundance, nitrate of baryta sulphates feebly, oxalate of ammonia lime sparingly, and phosphate of ammonia magnesia faintly. The original water had no action at all on lead. The lead plate became dull in a few hours, but no other change ensued which the eye could discover; and in twenty-three days the plate, which originally weighed 405·73 grains, weighed 405·74 grains.

These differences between the bottom and surface waters were so great, that it became desirable to repeat the examination, which I was able to do on the 6th of the present month. A good deal of easterly rain had fallen for some days until two days before this visit; but the hill streams had already become low. The waters were collected near the same place as before,—the bottom water from a depth of 94 fathoms, or 564 feet. The cistern brought up, as formerly, some peaty-like matter, which speedily subsided, and was promptly removed by decantation. Both specimens of water were very pure. But the bottom water was more affected than the surface water both by nitrate of silver and by oxalate of ammonia, and its colour was decidedly deeper, so that fully more than half its volume of colourless distilled water required to be added, to produce the feeble tint of the water from the surface.† The peaty matter

* 13,125 grains left 0·82 grains at 300, and 0·55 after incineration.

† The cistern which brought up the water was new, made of copper, and urnished, for valves, with spherical copper balls resting on hemispherical beds,

was found by microscopical examination to abound in diatoms and skeleton tissues of graminaceous and other vegetables. The bottom water contained a 25,000th of solids.

It has been proposed, in projects for introducing lake water into a town for domestic uses, to draw the water from a considerable depth, instead of from the surface, under the supposition that the deep water is the purest. The preceding observations show that this is a mistake, at least in the case of some lakes. On every occasion I have found the water of Loch Lomond somewhat more saline in its deepest parts than at the surface immediately above, and decidedly more coloured. The cause is easily understood, if the preceding chemical examination be taken in connection with the observations to be subsequently made on the temperature of Loch Lomond at various depths. For the results of both inquiries concur in indicating that, in the very deep parts, there is a vast body of still water which undergoes little, or, perhaps, no change or movement, and which, therefore, at the bottom, will become impregnated with whatever is soluble in the bed on which it rests.

Let me now change the scene to the hills and the waters of the Lowlands.

In the course of late notorious proceedings in this city for obtaining a more abundant water supply, it was stated by good chemical authorities that the water of St Mary's Loch in Selkirkshire, although of remarkable purity, does not exert upon metallic lead that eroding action which is a singular property of all pure waters previously subjected to trial. This statement was so opposed to the principles regulating the action of waters upon lead, as propounded by me so long ago as 1829, and also to the facts brought forward both then and in a paper read to this Society in 1842, that I resolved to investigate the question for myself.

This undertaking, in spite of my strong repugnance and steady refusal to be involved on either side of the Edinburgh water-controversy, led indirectly to my being compelled to concern myself with it as a parliamentary witness. But let it be clearly understood

and it was never used except for these experiments. The cistern was emptied at once into stoppered bottles on being drawn into the boat, and was carefully dried in a current of air with the valves open.

that my inquiries were undertaken quite irrespective of all controversial proceedings, parliamentary or otherwise, and for a purely scientific object—in which point of view alone I shall now proceed to state them. In the present place, I shall notice the lead question slightly, reserving that inquiry for another head of my observations. At present I have to say a few words of other matters which arose incidentally before me in the course of my inquiries.

St Mary's Loch is a lonely lake, retired among the hills of Selkirkshire, 37 miles south from Edinburgh. It is three miles long, and about half a mile in width at its broadest parts; but it may be said to be prolonged nearly another mile by the Loch of the Lowes above it, which is separated only by a space of 150 yards, through which the upper loch is joined to St Mary's Loch by a small stream. The lake in most parts shelves rapidly to a depth of 30 or 40 feet; in various parts it is said to deepen to 80, 100, and even 150 feet; and at a place pointed out to me as the deepest, I found 144 feet of water. It discharges itself in a goodly body of water, by a broad, shallow outlet to constitute the Yarrow Water. This joins the Ettrick a mile and a quarter above Selkirk; and the united waters, under the name of Ettrick, are poured, after a course of about four miles more, into the river Tweed. The Yarrow runs over 11 miles in a right line, but 14 miles by its windings, in a very stony channel, obviously of great width in floods.

The country of the Yarrow and St Mary's Loch is almost entirely pastoral, except where covered at the lower end of the stream by the beautiful woods of Bowhill, Philipshaugh, Hangingshaw, and other country seats. Around the lake itself the land may be described as consisting purely of pastoral hills, the attempts at arable culture being as yet very limited, and wood hitherto a scanty and stunted ornament. The level of the lake is almost exactly 800 feet above the sea. It is bordered everywhere, and abruptly, by hills rising from 750 to 1000 feet above it, showing long sky-lines, and steep slopes which present no rocks, no woods, nothing but smooth grass, unbroken save where scored by a few stream courses, mostly waterless in dry weather. But the Meggat Water is a considerable permanent stream, seven miles in direct length, which falls into St Mary's Loch about its middle line on the north; and the Little Yarrow, three miles in direct length, feeds the Loch of

the Lowes at its upper end. These streams, though short, are voluminous, because constantly supplied by numberless hill tributaries.

A traveller on the loch-side sees no peat anywhere. The district was therefore pronounced by recent one-eyed visitors to be free from peat. An inquisitive observer might have suspected the reverse from one of the highest surrounding hills being called Peat-Law; and on the high sky-line of another, a telescope would have betrayed to him a very suspicious circumstance in a crowd of little peat-stacks. Any one, not content with creeping along the bottom of valleys, but familiar with the summits of the mountains of the Scottish Lowlands, would then have known that the sky-line seen from the loch-side is not,—as it very often is in the primitive mountains of the Highlands,—a mere ridge, but forms the edge of a great table-top, which, in most cases, is chiefly composed of peat. In point of fact Professor Geikie has shown last summer, from the Government Geological Survey, that a vast proportion of the hill-tops in the St Mary's district consists of peat table-lands.

The consequences which flow from this structure of the country are peculiar. In dry weather the high peaty summits of the hills will cease to supply moisture enough to drain into the streamlets which score their sides. These will then convey to the lake chiefly the drainage of the grassy slopes, and the produce of the scanty springs in the lower regions. But when a rain-flood sets in, the peat, whether previously dry or moist, will send down a profusion of peaty water. Had the Yarrow flowed as a river through the vale at St Mary's, the peaty flood would have been swept quickly down towards the sea; and in two or three days the waters would have recovered from their peaty impregnation. But the two lochs, with a superficial area of two square miles, store up the peaty water, and dole it out, like a compensation pond, for many days, until the arrival of a fresh flood to renew it. An embankment at the outlet, to increase the storage, would protract the outflow, and postpone still further the recovery of the water from impurity.

These facts and views could only occur to one familiar with the district, or going thither to study it for a practical object. When I first went to St Mary's Loch on the 12th and 13th June last, I

had no further acquaintance with the hill structure around than that of an angler thirty years ago, when I probably looked more at what came out of the loch than at anything else concerning it. I consequently went prepossessed in its favour by the glowing account given of its extreme purity by its admirers. My surprise, therefore, was not small when my very first observation showed that its water was yellow. My visit was made in circumstances highly favourable to its condition, in splendid sunshine, being the last two days of six weeks of extraordinarily dry weather, broken only by a few light showers, sufficient to freshen the grass, and little more. But I found that my white porcelain basin became at once yellowish when dropped into the lake, acquired a lively amber hue at the depth of 3 feet, and disappeared entirely at 12 feet, while the sun shone brightly on the spot. I remembered well, however, having once distinguished small pebbles in the Dumfriesshire Esk through 16 feet of water, when spearing salmon in a still pool, and on another occasion through 21 feet in a pool below the Bracklinn Falls, near Callander. I afterwards tested the colour of the loch water on a small scale, and showed it satisfactorily to many, by comparing it with the water of Edinburgh of the same date in two narrow glass jars, 20 inches in height, with a circular disc of white porcelain at the bottom. The porcelain was of unstained whiteness as seen through the Edinburgh water, but of a lively amber tint when looked at through the water of St Mary's Loch. The difference was not less marked in the narrow 16-inch tubes. Even in dining-table water-bottles, placed on a white tablecloth, the colour of the loch water was such as to make it evident, that certainly nobody would drink it who could get the other. I may add that, when I revisited the loch on 8th September, also in bright sunshine, I found that my porcelain basin disappeared entirely in eight feet of water; and, nevertheless, there had been previously ten continuous days of absolutely dry weather.

On the 12th and 13th June, I saw in the water no want of the water-fleas, which excited so much interest and heat in the late controversy. It may create additional interest with some to be told that three months later they were decidedly bigger, busier, and altogether more deserving of their vernacular name.

Before speaking of the chemical composition of the water, let

me finish what may be said of the physical characters of the loch, by noticing one not yet adverted to. Visitors in the dry season, when the waters of the lake are somewhat shrunk, have been much struck with the beauty of its border,—its "silver strand." This is owing to a uniform beach of crowded, chiefly angular, or partially rounded, light-grey coloured stones. The colour, however, is not their own, but belongs to a generally dense covering of a dried-up matter, composed of a multitude of various diatoms entangled in the delicate lines of a finely fibrous conferva. In the fresh state this investing matter is dark greenish-brown, close, and slimy. The stones, therefore, give the loch, even in its shallows, a disagreeable, dark, deep appearance, abruptly defined by the water's edge. But all of them out of water acquire, in drying, a light grey or greyish-white hue. Every scientific visitor has observed, and some have carefully examined, these stones and their covering. But, so far as I am aware, no one has noted their full significance; of which more presently, when I come to speak of the Yarrow.

The water of the loch, though it is coloured, is a pure water,—in the sense that it contains very little solid matter in solution. It has been repeatedly analysed, and found to contain rather less than a 20,000th part of total solids. Mr Dewar, the latest analyst, I believe, found a 22,440th,—of which the inorganic salts constituted two-thirds [a 37,000th], and the organic matter one-third [a 55,500th]. The chief inorganic salts are the same as in the mica-slate streams and lochs of the Highlands, and much in the same proportion to one another. The hardness of the water was found by Mr Dewar to be 1·30 degrees by the soap-test, or nearly twice that of Loch Lomond surface water. Other chemists have found more solids, some less. My own results, with water collected on 13th June, show more saline, and rather less organic, matter; which is no more than might have been anticipated from the long antecedent very dry weather. I found the solid contents dried at about 300° F. to be a 15,000th of the water; one-fourth of this was destroyed by slow incineration at a low red heat; and the hardness was 2·0 degrees of Clark's soap-test scale,—which is about the fourth part of that of the present Edinburgh water supply. Water collected three months later, on 8th September, after ten days of complete drought, which, after a few days of showery weather,

followed the very heavy floods of 24th August, contained more colouring matter, exhibited less action with the ordinary liquid tests for the inorganic salts, and had a hardness of 1·4 degree only. I have no doubt that this water corresponded in all respects very closely with the specimen examined by Mr Dewar.

Thus, it appears, that the waters of St Mary's Loch—which, with the exception perhaps of those in the primitive districts of Kirkcudbrightshire and Wigtownshire, may be taken as a type of the lowland lochs at large—differ from the waters of the Highland lakes in containing more solid matter, a little more saline matter, and decidedly more colouring organic matter, and in being considerably harder, though really belonging to the "soft" waters too. Another difference is that they vary more with the season, the salts becoming rather more abundant in long dry weather, and the colouring matter clearly abounding more during and after floods. Finally, a remarkable difference in property, to be discussed by-and-by, is, that unlike the waters of the Highland lochs, that of St Mary's Loch does not erode lead. But first let me say a word or two about the Yarrow Water, by which this lake discharges itself.

The Yarrow, before uniting with the Ettrick, winds for 14 miles through a narrow, bare, chiefly pastoral vale, bounded by gently sloping hills. It is joined in this course by twenty-two tributaries, of which only three or four are considerable streamlets, the others being mostly rills, apt to be dried up, or nearly so, in dry weather. The waters of the chief tributaries contain in the dry season more salts than the main stream itself, but very much less colouring matter, two of them, indeed, none at all appreciable even in a 16-inch tube. The channel of the Yarrow is wide and stony, and the stream shallow, and for the most part turbulent. In the 14 miles it falls 220 feet. Its banks present very few human habitations.

These circumstances are favourable to the gradual diminution of organic impregnations, partly through the decomposing influence of fresh earthy salts added here and there by little tributaries, partly by the slow oxidation, to which Liebig gave the name of "Eremacausis,"—"quiet" or "slow burning." My attention was turned very long ago, before the publication of Liebig's views on this subject, to the rapidity with which, by natural processes,

streams rid themselves of the unnatural impurities introduced into them by sewage, and by some of the manufactures. But I am not aware that the process of clearing has been watched with care in circumstances altogether natural. It occurred to me, at anyrate, that we have in the Yarrow a most favourable opportunity for tracing this process in the case of a natural water of a remarkable kind, under the operation of natural causes alone. On the 8th of September, therefore, I examined the course of the Yarrow with some attention.

In its descent from St Mary's Loch, it is first joined by two unimportant rills, at that time nearly dried up by ten days of previous drought. A mile and a half below its outlet, it receives from the north its largest tributary, the Douglas Burn, which drains a very hilly country about five miles and a half long and four miles wide. This stream, indeed, was at the time a small rill, compared with the strong body of water in the Yarrow. But it was interesting in this respect, that its water, containing more saline matter than the main stream, and possessing the hardness of 4·90 degrees, presented no colour at all, even when examined in a 16-inch tube. This last fact is remarkable, because the Douglas Burn comes very much from peat-topped hills, so that either the peaty water of floods soon runs out in dry weather, and spring-water is alone left, or the water clears itself by eremacausis, or in its upper course in the way in which purification seems to be brought about in the Yarrow.

For, when I came to examine the Yarrow immediately above the junction of the Douglas Burn, I found to my surprise that the colour, which at the outlet was such as to render a porcelain basin invisible when sunk 8 feet only, was already so much reduced, in the course of a mile and a half, as to approach the faint hue of the waters of Loch Katrine and Loch Lomond. There was also a slight increase of salts, as shown by the ordinary liquid tests, and also by the hardness of the water having increased from 1·4 to 2·40 degrees.

A mile lower down another principal tributary, but inferior to the Douglas Burn, falls into the Yarrow on the right, the Altrieve Burn, which, however, I had not time enough to examine. Two miles further on a similar streamlet joins from the right, the

Sundhope, which, too, I could not examine. Other trifling rills, almost dried up, join between the Douglas Burn and Yarrow kirk, seven miles from the outlet of the lake. This point was a good one for studying the joint effect of atmospheric exposure through constant agitation, and of the influx of several brooks, all probably containing more salts than the main stream itself. Here I found that the soap-test indicated a further increase of hardness to 3·0 degrees, and that the yellow colour in a 16-inch tube was still further reduced, but not much.

In the next three miles and a half there are six little tributaries, all at the time of my visit insignificant, and some quite dried up, till we arrive at the Lewenshope Burn, which drains from the north a considerable stretch of the Minchmoor range, described to me as generally stony hills, without much peat. This water possessed 6·5 degrees of hardness, and so little colour that it was barely appreciable in a 16-inch tube. In the remainder of its course the Yarrow is joined by five more rills, either almost dried up when I was there, or appropriated in a great measure for the supply of mansions. Four hundred yards above its junction with the Ettrick, I found its water to possess, as at Yarrow kirk, seven miles higher up, 3·0 degrees of hardness, so that the comparatively saline water of the Lewenshope had not materially increased the salts of the Yarrow. But the colour was still more reduced, so as to be very faint indeed, equally so with the colour of the water of Loch Lomond.

Thus the principal loss of colour takes place in the first mile and a half of the river's course; but there was also a very appreciable additional improvement in the longer course below, and the final result was a nearly total removal of colour.

To what is this change owing? Does it depend entirely on the intermixture of earthy salts from the tributaries, and on eremacausis? I apprehend that these causes will scarcely account for the great change effected in the first mile and a half. There may even be a doubt whether peat-extract is particularly subject to the process of eremacausis. It is well known to be a preservative of organic matters, which it could scarcely be were it very subject to decay itself; and I find that a solution of it without any saline matter, has undergone no change in a warm room, in a half-filled

bottle, during six months. But there is a more potent agent at work in the Yarrow. The dark, green-coated stones of the loch, with all their characters unreduced, pave the entire channel of the stream as low at least as the confluence of the Douglas Burn, and, with a less abundant covering, so low at least as Yarrow kirk, seven miles from the outlet of the lake. But there is nothing of the kind in the chief tributaries. At the junction, for example, of the Douglas Burn, there is an abrupt line of demarcation between the dark green, slippery stones of the Yarrow, and the stones of the tributary, which are as naked as if they had been scrubbed clean with a brush. I do not well see how to escape the conclusion, that the confervæ and diatoms of the stones live at the cost of the peaty matter from the loch,—that peat-extract is their food and is consumed by them. This is a ready explanation of their excessive growth on the stones of the loch. The want of such food equally explains the comparative absence of them from the stony banks of Loch Lomond, and the stony channels of all the streams of the adjacent mica-slate district.* Indeed, in the opposite circumstance—in some mountain tarns of the district, resting, as they may, on peat, and surrounded by it—the slippery, dark green, stony bottom is no uncommon occurrence.

If these views be correct, it is easy to appreciate both the unfavourable significance in a lake of a dark-green bottom of stones, densely covered with confervæ and diatoms, and likewise their value in a running stream; and it may be well also not to let the imagination run away luxuriating in every "silver strand" that meets the eye.

The Temperature of the Deep Fresh-water Lakes of this country has no connection with the preceding inquiries, further than that my observations on the subject arose incidentally while I was carrying on the inquiries in question. The results I have obtained may interest the cultivator of physical geography, if I am right

* It has been said that stones covered with green confervæ and other diatoms do occur in Loch-Lomond. They do in bays and other shallows; but the covering is very thin; and the line of such stones is narrow. Where deep water is near there are none at the edge, and where they do occur the dry stones close to the edge appear quite clean.

in supposing that no prior observations of the kind have been made on our deep fresh waters. [See, however, p. 574.]

In the course of the discussion of the St Mary's Loch water-supply scheme, opposite opinions were expressed as to the relative advantage of drawing the water from the surface of the lake, or from a considerable depth; and weighty arguments, of a speculative nature, were advanced on both sides of the question. It occurred to me, therefore, to consider what becomes of the deep water. Does it escape as that of the surface must do? And if so, How? It appeared to me that during a winter of such protracted cold as that of 1870–71, the water at the bottom would probably acquire so low a temperature, that it must long remain there. For it can only rise again, either by its temperature falling below 39°·5, when its density decreases instead of continuing to increase, or by being heated by the heat of the earth beneath; and it is unlikely that the temperature of the entire water of a deep lake will fall lower than 39°·5, or indeed so low, in this latitude, and the heat derived from the earth, in our latitude at the elevation of 800 feet above the sea, must be inconsiderable. It is well known that the bottom cannot be heated by conduction from the summer heat of the atmosphere above, as in the case of a solid substance; and the effect of the penetration of the sun's rays, by which the water is heated to a certain depth, cannot descend very low in a lake, the water of which is, like that of St Mary's Loch, so coloured as to render a very white object invisible at the depth of 8 or 12 feet. The conclusion would be that the water at the bottom of the deep parts of the lake, in the absence of strong springs—of the existence of which there is neither proof nor probability—will remain at the bottom for want of a current during the whole warm season, and perhaps longer.

When I was first at St Mary's Loch on 12th and 13th June, I had no suitable thermometer for taking observation of deep temperatures. But Mr Dewar kindly undertook to make the necessary trial a few days later in the same month. With a Six's thermometer, whose graduation was subsequently tested and found correct, he ascertained that in 150 feet soundings, the temperature, being 56 at the surface, was 46° at the bottom. When I revisited St Mary's Loch on 8th September, nearly three months afterwards, the inter-

mediate weather having been generally fine, I found, with the same thermometer, in 96 feet of water, near the head of the lake, 56° at the surface and 54° at the bottom; and in 144 feet of water, in the middle of the loch, exactly opposite the 17th milestone from Selkirk, I obtained 55° at the surface and 47° at the bottom. During three of the warmest months of last warm season, the heat of the earth, or the sun's rays, had heated the water at the bottom by one degree of Fahrenheit only. I do not well see how that water can ever rise from such a depth, unless its temperature during the winter should fall below 39°·5, which is not probable.

I regret I did not take successive observations at several depths in order to fix the upper limit of the cold substratum of water. My time was short, for my main object on that occasion was the changes undergone by the river Yarrow, and I contemplated a chain of observations in more favourable circumstances at Loch Lomond. I went to Loch Lomond on four occasions for the purpose, viz., on September 14th, September 21st, October 11th and 12th, and November 18th. As accurate observations were made only on the two last occasions, I shall refer to the others only incidentally.

On 11th October, at 3 P.M., the atmospheric temperature on land being 48°, and that of the surface water everywhere over deep soundings 52°, I found in 103 fathoms of water opposite Culness, with a Six's thermometer by Casella, which, though not specially protected against high pressure, was believed to be proof against such pressures as it was to be subjected to, that a temperature of 43° was indicated at 200 feet, and 41°·8 steadily at 400, 500, and 618 feet. Next forenoon at 11, I repeated my observations about a mile lower down opposite Tarbet in 87 fathoms. The air was singularly still, the atmospheric temperature on land 44°, and that of the loch on the surface 52°, exactly as on the previous day. The following successive temperatures were obtained at various depths:—

Surface,	52°·0	150 feet,	44°·5
25 feet,	51°·5	200 „	43°·0
50 „	50°·2	300 „	42°·0
75 „	50°·0	400 „	42°·0
100 „	49°·5	518 „ bottom,	42°·0

It will be observed that these temperatures correspond almost exactly with such observations of the previous day as were made a mile and a half further north at the same depths, where the soundings were 618 fathoms. The bottom temperatures also corresponded with what I had observed with a different thermometer on September 21st, three weeks earlier. Using a cistern with proper valves, constructed by Mr Adie, for bringing up 96 ounces of water from the bottom, with a simple thermometer in it, I found that on September 21st, when the surface temperature was 54°, and also on October 11th, when it was 52°, the thermometer, on the instrument arriving at the surface, indicated 44° in the water brought up from the bottom, both in 87 and 103 fathoms of water. As the heating of the cistern in ascending must have been very nearly or altogether the same on both occasions, it follows that the corrected temperature at the bottom, as on 11th October, was 42° on 21st September.

On 18th November I found it to be also the same. Cold weather had set in for a week before. The air was frosty, the ground dry and hard, the atmosphere very clear and perfectly still. Near the lower end of the loch, where the highway first touches it, the air temperature was 33° at half-past eleven. At Tarbet at one P.M., it was on land, but at the water's edge, 37°; in the boat, in the middle of the loch, two feet above its surface, 42°; and in surface water, over 610 feet soundings, 46°. At the bottom, by a Casella's thermometer, protected against pressure, and corresponding exactly in its graduation with the unprotected one previously used, the bottom temperature was again 42°. My design to make at the same time another complete series of observations, was prevented by unexpected delays shortening my time very much, so that I had to confine myself to a single additional observation, for determining more nearly the upper limit of the cold substratum of water. At 250 feet I obtained a temperature of 42°·25, and consequently the upper limit of the water at 42° must have been as nearly as possible at 270 feet in 610 feet soundings.

Before drawing confident deductions from these observations, they require to be repeated at other seasons. But in the meanwhile it may be well to see what are likely to be the results.

It is plain, in the first place, that in a deep lake in this latitude, there is a very gradual and slight increase of cold in the warm season for the first hundred feet, viz., by 2°·5 only, then a sudden descent by 5°·0 in the next 50 feet only; next another slow descent by 2°·5 in 150 feet; and finally, below that a great substratum of 250 feet of water, and at a deeper spot of no less than 350 feet, at the uniform temperature of 42°, or a little less. Next, at Loch Lomond no change took place in the temperature of the bottom water during two months of unusual warmth for the months of September and October, and no change at 300 feet from the surface during five weeks prior to the middle of November.

It seems certain that the temperature of the great substratum of cold water cannot be raised after the middle of November, when the cold season has fairly set in. Whether it is to be lowered during winter, or whether the substratum, without becoming colder, will merely have its upper level raised, is a question to be settled by observation at an early period of next spring.

In the meanwhile, abstracting the highly improbable existence of strong springs at the great depths I have mentioned, it does not appear how this vast cold substratum could have been moved during last summer and autumn. Neither does it appear how it can be moved during the winter, unless the equally great stratum above it acquire a lower temperature than 42°, and so take its place; for the uniformity of the bottom temperature between 21st September and 18th November, when no additional cold could descend through the warmer stratum above, is sufficient proof that the influence of the heat of the earth beneath is too feeble in this latitude to make itself sensibly felt by motion of the water.

Thus there is a probability, that when water once descends to so great a depth as the bottom of our deep lakes, it cannot ascend again except under rare and extraordinary circumstances. If this view be correct, the movement of the waters of a deep lake towards its outlet for escape, must be confined very much to the warm water at its surface, or to no great depth, and, therefore, mainly to the waters which are constantly supplied on all sides by its feeding streams. This must be the case in summer and in autumn; it may be the case in winter also

[*May* 18, 1872.—Circumstances having delayed the publication of the Society's Proceedings, I take this opportunity of adding the result of recent and conclusive observations. These were made on 10th April and 6th May, as near as I could to the place of the observations described above.

April 10.—The weather on this occasion was very fine and favourable for my purpose. During the whole winter period after November 18th, the date of the last observations, the weather had been remarkably open. The mean temperature of the atmosphere for the five intervening months, as kindly calculated for me by Mr Buchan, Secretary of the Meteorological Society, from observations at Balloch Castle, at the southern end of the loch, was 1°·4 higher than the average for the same months for thirteen previous years.* Consequently, the same influence of the winter season on the temperature of deep waters cannot be expected as in ordinary winters, or in a hard winter, such as the preceding one of 1870–71.

When I made my observations, about 3 P.M. on 10th April, the temperature of the air on land was 55°; and on the water, one mile from the shore whence the wind blew, it was 53° in the boat, scarcely 2 feet above the surface of the lake. The following temperatures were obtained, at various depths in the same place:—

Surface, . .	43°·0	150 feet, . .	42°·1
50 feet, . .	42°·6	200 " . .	42°·0
75 " . .	42°·2	594 " bottom,	42°·0
100 " . .	42°·2		

These observations were made with Casella's protected thermometer. The thermometer in Adie's cistern, for bringing up water from the bottom, also stood at 42° when brought up to the surface, the temperature of the upper warmer stratum being much too low to affect the cistern in its passage.

May 6.—Between 10th April and this date the weather varied

* In the course of his calculations Mr Buchan arrived at the interesting fact that the average mean temperature of the air during the six cold months of these years, at the level of the lake's surface, was 41°·7 from November 18 to April 10, or very nearly that of the deep substratum.—*See subsequently, for his observations, the later Proceedings of the Society.*

as to warmth; but there was a large proportion of sunshine, and little rain, till three days before, when there was a heavy fall with an easterly wind. The temperature on land, within fifty yards of the water, was 55°. The following observations were made at 2 P.M.:—

Surface, . .	44°·5	150 feet, . .	42°·7
25 feet, . .	43°·7	175 " . .	42°·6
50 " . .	43°·5	200 " . .	42°·5
75 " . .	43°·2	250 " . .	42°·4
100 " . .	43°·1	300 " . .	42°·1
125 " . .	42°·8	574 " . .	42°·1

The thermometer in Adie's cistern, when brought up full of water from the bottom, but raised rather deliberately, stood at 42°·5.

It appears, from these and the preceding observations, that in the deep parts of Loch Lomond there is a substratum of water of several hundred feet, which, between the end of September last and 10th April, has been steadily of the temperature of 42°; and that during last winter no other change has taken place, in relation to temperature in or near it, than that the level of the cold substratum rose in the interval between 70 and 100 feet. A winter, materially colder than the last unusually mild one, would at least raise that level still nearer the surface. Whether it may reduce the temperature still lower than 42°, is a question which remains to be decided by future observation. It is still also a matter for observation, whether the temperature of the substratum may not rise a little during summer. For it may be reasonably said, that the unusually hard winter of 1870–71 might have lowered the temperature of the substratum in April of last year below that observed in April of this year after a very open winter, and, consequently, under 42°, which was the temperature observed in October. But the difference, if any, cannot be considerable; for it can only arise from the heating power of the earth on which the water rests.

The water of a lake is heated in summer and autumn in three ways—the heat of the atmosphere, that of the sun's rays, and that of the earth. The atmosphere will communicate its heat to so much of the superstratum only as is disturbed, more or less, by the wind; and, therefore, cannot penetrate many feet. The tempera-

ture of the earth at the bottom, from 500 to 600 feet under the sea-level, should be by theory about 60° in the deepest parts; but, considering the very low conducting power of the rocky structure of the earth, its heating power over so vast a bed of cold water must be very feeble. The sun's rays are at once the most energetic heating power, that which penetrates deepest, and that which alone can sensibly heat any part of the superstratum of water underneath the thin bed near the surface, where it is aided by the warmth of the atmosphere, and the stirring of the water by the wind. But there is a limit to the sun's penetration in such depths, when the water, as in the case of Loch Lomond, is coloured, however slightly. It has been imagined that the presence of springs at the bottom may be a fourth source of influence over the temperature. If there be any springs there, the effect must be to heat the water. But, as there are no springs in Scotland which rise above the surface, or present other proofs of owing their place to unusual sources of pressure, it seems most improbable that any are so constituted as to overcome the pressure which exists at the bottom of a very deep lake.

Every known consideration,—the great thickness of the cold substratum, its steady low temperature, and its greater colour than at the surface—contributes proof that this substratum can undergo little or no movement, unless an unusually hard winter should displace it by colder water from above.*]

The previous observations have extended to so great a length that I must postpone till another opportunity the remarks which I have prepared on the third of my promised topics—the Action of Water on Lead.

The following Gentlemen were elected Fellows of the Society:—

Alexander H. Lee, Esq., C.E.
Robert Lee, Esq., Advocate.
John Anderson, LL.D.

* While the preceding statements were passing through the press, my attention was called to similar observations in Sir John Leslie's article on Climate in the "Encyclopædia Britannica," by Saussure on the Lakes of Geneva, Thun, and Lucerne, and by the late eminent engineer, Mr James Jardine, on Loch Lomond and Loch Katrine in 1814. Their observations are not entirely concordant with those given above. I contemplate further observations which may reconcile them.

Monday, 18th December 1871.

Sir ROBERT CHRISTISON, Bart., President, in the Chair.

The following Communications were read:—

1. On the Computation of the Strengths of the Parts of Skeleton or Open Structures. By Edward Sang.

The first part of the paper is devoted to the computation of the strengths of the parts of a structure destined to resist given strains, taking into account, along with those strains, the unknown weights of the parts. The results obtained by this process necessarily give the best possible arrangement of the strengths, since, if any one part were made weaker, the whole structure would be weakened; or, if a part were made stronger, the unnecessary weight thus thrown upon the other parts would also go to weaken the fabric. It is believed that this investigation has now been given for the first time.

It was pointed out that this method enables us to determine the utmost limit of magnitude of a structure having a given general configuration.

The second part concerned deficient or flexible structures; the mode of discovering the relations among the applied pressures, needed to cause the structure to assume a prescribed form, was indicated.

Thirdly, the case of redundant structures was gone into. It was observed that the absolute strains on the parts of such structures depend, not merely on their form, but also on the manner of putting them together. The changes on these strains caused by additional loads can, however, be computed by considering the compressions or distensions of the parts; and it was pointed out that the computation of these changes has been mistaken for that of the absolute strains.

Lastly, there was investigated a new general theorem, which may be stated as follows:—

When we apply a pressure to some point of a flexible system,

the yielding is not necessarily in the direction of the pressure. There is, however, always one direction of coincidence, and there may be three. When there are three, if two of these form a right angle, the third is also perpendicular to both of them.

2. On Vortex Motion. By Professor Sir William Thomson.

(*Abstract.*)

This paper is a sequel to several communications which have already appeared in the Proceedings and Transactions of the Royal Society of Edinburgh.* It commences with an investigation of the circumstances under which a portion of an incompressible frictionless liquid, supposed to extend through all space, or through space wholly or partially bounded by a rigid solid, can be projected so as to continue to move through the surrounding liquid without change of shape; and goes on to investigate vibrations executed by a portion of liquid so projected, and slightly disturbed from the condition that gives uniformity. The greatest and least quantities of energy which a finite liquid mass of any given initial shape and any given initial motion can possess, after any variations of its bounding surface ending in the initial shape, are next investigated; and the theory of the dissipation of energy in a finite or infinite frictionless liquid is deduced. A finite space, filled with incompressible liquid, traversed by a great multitude of parts of itself, each very small in comparison with the average distance of any one of the parts from its nearest neighbour, is next considered, and thus a kinetic theory of gases, without the assumption of elastic atoms, is sketched; also a realisation by vortex atoms of Le Sage's "gravific" fluid consisting of an innumerable multitude of "ultramundane corpuscles."

Towards the vortex theory of the elasticity of liquids and solids, the propagation of waves along a row of vortex columns alternately positive and negative, in a liquid contained between two rigid parallel planes, close enough to give stability to the row of columns, is next investigated.

In conclusion, it is pointed out that the difficulties of forming a complete theory of the elasticity of gases, liquids, and solids, with

* Vortex Atoms. Proceedings, February 1867; Transactions, 1868–1869.

no other ultimate properties of matter than perfect fluidity and incompressibility are noticed, and shown to be, in all probability, only dependent on the weakness of mathematics.

3. On the Ultramundane Corpuscules of Le Sage. By Professor Sir W. Thomson.

(*Abstract.*)

Le Sage, born at Geneva in 1724, devoted the last sixty-three years of a life of eighty to the investigation of a mechanical theory of gravitation. The probable existence of a gravific mechanism is admitted and the importance of the object to which Le Sage devoted his life pointed out, by Newton and Rumford* in the following statements:—

"It is inconceivable that inanimate brute matter should, without
" the mediation of something else, which is not material, operate
" upon, and affect other matter without mutual contact; as it must
" do, if gravitation, in the sense of *Epicurus*, be essential and
" inherent in it. And this is the reason why I desired you would
" not ascribe innate gravity to me. That gravity should be innate,
" inherent, and essential to matter, so that one body may act upon
" another at a distance through a *vacuum*, without the mediation
" of anything else, by and through which their action and force
" may be conveyed from one to another, is to me so great an
" absurdity, that I believe no man who has in philosophical
" matters a competent faculty of thinking, can ever fall into it.

* On the other hand, by the middle of last century the mathematical naturalists of the Continent, after half a century of resistance to the Newtonian principles (which, both by them and by the English followers of Newton, were commonly supposed to mean the recognition of gravity as a force acting simply at a distance without mediation of intervening matter), had begun to become more "Newtonian" than Newton himself. On the 4th February 1744, Daniel Bernoulli wrote as follows to Euler, "Uebrigens glaube ich,
" dass der Aether sowohl *gravis versus solem*, als die Luft versus terram
" sey, und kann Ihnen nicht bergen, dass ich über diese Puncte ein völliger
" Newtonianer bin, und verwundere ich mich, dass Sie den Principiis
" Cartesianis so lang adhäriren; es möchte wohl einige Passion vielleicht
" mit unterlaufen. Hat Gott können eine *animam*, deren Natur uns unbe-
" greiflich ist, erschaffen, so hat er auch können eine attractionem universalem
" materiæ imprimiren, wenn gleich solche attractio *supra captum* ist, da
" hingegen die Principia Cartesiana allzeit *contra captum* etwas involviren."

"Gravity must be caused by an agent acting constantly accord-
"ing to certain laws; but whether this agent be material or
"immaterial, I have left to the consideration of my readers."—
Newton's Third Letter to Bentley, February 25th, 1692–3.

"Nobody surely, in his sober senses, has ever pretended to "understand the mechanism of gravitation; and yet what sublime "discoveries was our immortal Newton enabled to make, merely "by the investigation of the laws of its action." *

Le Sage expounds his theory of gravitation, so far as he had advanced it up to the year 1782, in a paper published in the Transactions of the Royal Berlin Academy for that year, under the title "Lucrèce Newtonien." His opening paragraph, entitled, "But de ce mémoire," is as follows:—

"Je me propose de faire voir: que si les premiers Epicuriens "avoient eu; sur la Cosmographie des idées aussi saines seule- "ment, que plusieurs de leurs contemporains, qu'ils négligeoient "d'écouter; † et sur la Géométrie, une partie des connoissances "qui étoient déjà communes alors: ils auroient, tres probablement, "découvert sans effort; les Loix de la Gravité universelle, et sa "Cause mécanique. *Loix;* dont l'invention et la démonstration, "font la plus grande gloire du plus puissant génie qui ait jamais "existé: et *Cause*, qui après avoir fait pendant longtems, l'ambition "des plus grands Physiciens; fait à présent, le désespoir de leurs "sucesseurs. De sorte que, par exemple, les fameuses Régles de "*Kepler;* trouvées il y a moins de deux siecles, en partie sur des "conjectures gratuites, et en partie aprés d'immenses tâtonnemens; "n'auroient été que des corollaires particuliers et inévitables, des "lumières générales que ces anciens Philosophes pouvoient puiser "(comme en se jouant) dans le méchanisme proprement dit de "la Nature. Conclusion; qu'on peut appliquer exactement aussi, "aux Loix de *Galilée* sur la chûte des Graves sublunaires; dont "la découverte a été plus tardive encore, et plus contestée: joint "à ce que, les expériences sur lesquelles cette découverte étoit "établie; laissoient dans leurs résultats (nécessairement grossiers),

* An Inquiry concerning the Source of the Heat which is excited by Friction. By Count Rumford.—*Philosophical Transactions*, 1798.

† Vobis (Epicureis) minùs notum est, quemadmodum quidque dicatur. Vestra enim solùm legitis, vestra amatis; cæteros, causâ incognitâ, condemnatis. Cicéron, De natura Deorum, ii. 29.

"une latitude, que les rendoit également compatibles avec plusieurs "autres hypothèses; qu'aussi, l'on ne manqua pas de lui opposer: "au lieu que, les conséquences du choc des Atoms; auroient été "absolument univoques en faveur du seul principe véritable (des "Accélérations égales en Tempuscules égaux)."

If Le Sage had but excepted Kepler's third law, it must be admitted that his case, as stated above, would have been thoroughly established by the arguments of his "mémoire;" for the epicurean assumption of parallelism adopted to suit the false idea of the earth being flat, prevented the discovery of the law of the inverse square of the distance, which the mathematicians of that day were quite competent to make, if the hypothesis of atoms moving in all directions through space, and rarely coming into collision with one another, had been set before them, with the problem of determining the force with which the impacts would press together two spherical bodies, such as the earth and moon were held to be by some of the contemporary philosophers to whom the epicureant "would not listen." But nothing less than direct observation, proving Kepler's third law,—Galileo's experiment on bodies falling from the tower of Pisa, Boyle's guinea and feather experiment, and Newton's experiment of the vibrations of pendulums composed of different kinds of substance—could give either the idea that gravity is proportional to mass, or prove that it is so to a high degree of accuracy for large bodies and small bodies, and for bodies of different kinds of substance. Le Sage sums up his theory in an appendix to the "Lucrèce Newtonien," part of which translated (literally, except a few sentences which I have paraphrased) is as follows:—

Constitution of Heavy Bodies.

1*st*, Their indivisible particles are cages; for example, empty cubes or octahedrons vacant of matter except along the twelve edges.

2*d*, The diameters of the bars of these cages, supposed increased each by an amount equal to the diameter of one of the gravific corpuscles, are so small relatively to the mutual distance of the parallel bars of each cage, that the terrestrial globe does not intercept even so much as a ten-thousandth part of the corpuscules which offer to traverse it.

3d, These diameters are all equal, or if they are unequal, their inequalities sensibly compensate one another [in averages].

Constitution of Gravific Corpuscules.

1st, Conformably to the second of the preceding suppositions, their diameters added to that of the bars is so small relatively to the mutual distance of parallel bars of one of the cages, that the weights of the celestial bodies do not differ sensibly from being in proportion to their masses.

2d, They are isolated. So that their progressive movements are necessarily rectilinear.

3d, They are so sparsely distributed, that is to say, their diameters are so small relatively to their mean mutual distances, that not more than one out of every hundred of them meets another corpuscule during several thousands of years. So that the uniformity of their movements is scarcely ever troubled sensibly.

4th, They move along several hundred thousand millions of different directions; in counting for one same direction all those which are [within a definite very small angle of being] parallel to one straight line. The distribution of these straight lines is to be conceived by imagining as many points as one wishes to consider of different directions, scattered over a globe as uniformly as possible, and therefore separated from one another by at least a second of angle; and then imagining a radius of the globe drawn to each of those points.

5th, Parallel, then, to each of those directions, let a current or torrent of corpuscules move; but, not to give the stream a greater breadth than is necessary, consider the transverse section of this current to have the same boundary as the orthogonal projection of the visible world on the plane of the section.

6th, The different parts of one such current are sensibly equidense; whether we compare, among one another, collateral portions of sensible transverse dimensions, or successive portions of such lengths that their times of passage across a given surface are sensible. And the same is to be said of the different currents compared with one another.

7th, The mean velocities, defined in the same manner as I have just defined the densities, are also sensibly equal.

8th, The ratios of these velocities to those of the planets are several million times greater than the ratios of the gravities of the planets towards the sun, to the greatest resistance which secular observations allow us to suppose they experience. For example, [these velocities must be] some hundredfold a greater number of times the velocity of the earth, than the ratio of 190,000* times the gravity of the earth towards the sun, to the greatest resistance which secular observations of the length of the year permit us to suppose that the earth experiences from the celestial masses.

CONCEPTION, *which facilitates the Application of Mathematics to determine the mutual Influence of these Heavy Bodies and these Corpuscules.*

1st, Decompose all heavy bodies into molecules of equal mass, so small that they may be treated as attractive points in respect to theories in which gravity is considered without reference to its cause; that is to say, each must be so small that inequalities of distance and differences of direction between its particles and those of another molecule, conceived as attracting it and being attracted by it, may be neglected. For example, suppose the diameter of the molecule considered to be a hundred thousand times smaller than the distance between two bodies of which the mutual gravitation is examined, which would make its apparent semi-diameter, as seen from the other body, about one second of angle.

2d, For the surfaces of such a molecule, accessible but impermeable to the gravific fluid, substitute one single spherical surface equal to their sum.

3d, Divide those surfaces into facets small enough to allow them to be treated as planes, without sensible error, [&c., &c.]

Remarks.

It is not necessary to be very skilful to deduce from these suppositions all the laws of gravity, both sublunary and universal (and consequently also those of Kepler, &c.), with all the accuracy which observed phenomena have proved those laws. Those laws,

* To render the sentence more easily read, I have substituted this number in place of the following words :—" le nombre de fois que le firmament contient le disque apparent du soleil."

therefore, are inevitable consequences of the supposed constitutions.

2*d*, Although I here present these constitutions crudely and without proof, as if they were gratuitous hypotheses and hazarded fictions, equitable readers will understand that on my own part I have at least some presumptions in their favour (independent of their perfect agreement with so many phenomena), but that the development of my reasons would be too long to find a place in the present statement, which may be regarded as a publication of theorems without their demonstrations.

3*d*, ... There are details upon which I have wished to enter on account of the novelty of the doctrine, and which will readily be supplied by those who study it in a favourable and attentive spirit. If the authors who write on hydro-dynamics, aerostatics, or optics, had to deal with captious readers, doubting the very existence of water, or air, or light, and therefore not adapting themselves to any tacit supposition regarding equivalencies or compensations not expressly mentioned in their treatises, they would be obliged to load their definitions with a vast number of specifications which instructed or indulgent readers do not require of them. One understands "*à demi-mot*" and "*sano sensu*" only familiar propositions towards which one is already favourably inclined.

Some of the details referred to in this concluding sentence of the appendix to his "Lucrèce Newtonien," Le Sage discusses fully in his "Traité de Physique Mécanique," edited by Pierre Prévost, and published in 1818 (Geneva and Paris).

This treatise is divided into four books.

I. "Exposition sommaire du système des corpuscules ultramondains."

II. "Discussion des objections qui peuvent s'élever contre le " système des corpuscules ultramondains."

III. "Des fluides élastiques ou expansifs."

IV. "Application des théories précédentes a certaines affinités."

It is in the first two books that gravity is explained by the impulse of ultramundane corpuscules, and I have no remarks at present to make on the third and fourth books.

From Le Sage's fundamental assumptions, given above as nearly as may be in his own words, it is, as he says himself, easy to deduce the law of the inverse square of the distance, and the law of proportionality of gravity to mass The object of the present note is not to give an exposition of Le Sage's theory, which is sufficiently set forth in the preceding extracts, and discussed in detail in the first two books of his posthumous treatise. I may merely say that inasmuch as the law of the inverse square of the distance, for every distance, however great, would be a perfectly obvious consequence of the assumptions, were the gravific corpuscules infinitely small, and therefore incapable of coming into collision with one another, it may be extended to as great distances as we please, by giving small enough dimensions to the corpuscules relatively to the mean distance of each from its nearest neighbour. The law of masses may be extended to as great masses as those for which observation proves it (for example the mass of Jupiter), by making the diameters of the bars of the supposed cage-atoms constituting heavy bodies, small enough. Thus, for example, there is nothing to prevent us from supposing that not more than one straight line of a million drawn at random towards Jupiter and continued through it, should touch one of the bars. Lastly, as Le Sage proves, the resistance of his gravific fluid to the motion of one of the planets through it, is proportional to the product of the velocity of the planet into the average velocity of the gravific corpuscules; and hence by making the velocities of the corpuscules great enough, and giving them suitably small masses, they may produce the actual forces of gravitation, and not more than the amount of resistance which observation allows us to suppose that the planets experience. It will be a very interesting subject to examine minutely Le Sage's details on these points, and to judge whether or not the additional knowledge gained by observation since his time requires any modification to be made in the estimate which he has given of the possible degrees of permeability of the sun and planets, of the possible proportions of diameters of corpuscules to interstices between them in the "gravific fluid," and of the possible velocities of its component corpuscules. This much is certain, that if hard indivisible atoms are granted at all, his principles are unassailable; and nothing can be said against the probability

of his assumptions. The only imperfection of his theory is tha which is inherent to every supposition of hard, indivisible atoms. They must be perfectly elastic or imperfectly elastic, or perfectly inelastic. Even Newton seems to have admitted as a probable reality hard, indivisible, unalterable atoms, each perfectly inelastic.

Nicolas Fatio is quoted by Le Sage and Prévost, as a friend of Newton, who in 1689 or 1690 had invented a theory of gravity perfectly similar to that of Le Sage, except certain essential points; had described it in a Latin poem not yet printed; and had written, on the 30th March 1694, a letter regarding it, which is to be found in the third volume of the works of Leibnitz, having been communicated for publication to the editor of those works by Le Sage. Redeker, a German physician, is quoted by Le Sage as having expounded a theory of gravity of the same general character, in a Latin dissertation published in 1736, referring to which Prévost says, "Où l'on trouve l'exposé d'un système fort semblable à celui " de Le Sage dans ses traits principaux, mais dépourvu de cette " analyse exacte des phénomènes qui fait le principal mérite de toute " espèce de théorie." Fatio supposed the corpuscules to be elastic, and seems to have shown no reason why their return velocities after collision with mundane matter should be less than their previous velocities, and therefore not to have explained gravity at all. Redeker, we are told by Prévost, had very limited ideas of the permeabilities of great bodies, and therefore failed to explain the law of the proportionality of gravity to mass; "he enunciated this law " very correctly in section 15 of his dissertation; but the manner " in which he explains it shows that he had but little reflected upon " it. Notwithstanding these imperfections, one cannot but recog- " nise in this work an ingenious conception which ought to have " provoked examination on the part of naturalists, of whom many " at that time occupied themselves with the same investigation. " Indeed, there exists a dissertation by Segner on this subject.* " But science took another course, and works of this nature gradu- " ally lost appreciation. Le Sage has never failed on any occasion " to call attention to the system of Redeker, as also to that of Fatio."†

* De Causa gravitatis Redekeriana.

† Le Sage was remarkably scrupulous in giving full information regarding ll who preceded him in the development of any part of his theory.

Le Sage shows that to produce gravitation those of the ultramundane corpuscules which strike the cage-bars of heavy bodies must either stick there or go away with diminished velocities. He supposed the corpuscules to be inelastic (*durs*), and points out that we ought not to suppose them to be permanently lodged in the heavy body (*entassés*), that we must rather suppose them to slip off; but that being inelastic, their average velocities after collision must be less than that which they had before collision.*

That these suppositions imply a gradual diminution of gravity from age to age was carefully pointed out by Le Sage, and referred to as an objection to his theory. Thus he says, "... Donc, la durée " de la gravité seroit *finie* aussi, et par conséquent la durée du " monde.

"*Réponse.* Concedo; mais pourvu que cet obstacle ne contribue " pas à faire finir le monde plus promptement qu'il n'auroit fini sans " lui, il doit être considéré comme nul."†

Two suppositions may be made on the general basis of Le Sage's doctrine:—

1*st*, (Which seems to have been Le Sage's belief.) Suppose the whole of mundane matter to be contained within a finite space, and the infinite space round it to be traversed by ultramundane corpuscules; and a small proportion of the corpuscules coming from ultramundane space to suffer collisions with mundane matter, and get away with diminished gravific energy to ultramundane space again. They would never return to the world were it not for collision among themselves and other corpuscules. Le Sage held that such collisions are extremely rare; that each collision, even between the ultramundane corpuscules themselves, destroys some energy;‡ that at a not infinitely remote past time they were set in motion for the purpose of keeping gravitation throughout the world in action for a limited period of time; and that

* Le Sage estimated the velocity after collision to be two-thirds of the velocity before collision.

† Posthumous. "Traité de Physique Mécanique," edited by Pierre Prévost. Geneva and Paris, 1818.

‡ Newton (Optics, Query, 30 Edn. 1721, p. 373) held that two equal and similar atoms, moving with equal velocities in contrary directions, come to rest when they strike one another. Le Sage held the same; and it seems that writers of last century understood this without qualification when they called atoms hard.

both by their mutual collisions, and by collisions with mundane atoms, the whole stock of gravific energy is being gradually reduced, and therefore the intensity of gravity gradually diminishing from age to age.

Or, 2*d*, suppose mundane matter to be spread through all space, but to be much denser within each of an infinitely great number of finite volumes (such as the volume of the earth) than elsewhere. On this supposition, even were there no collisions between the corpuscules themselves, there would be a gradual diminution in their gravific energy through the repeated collisions with mundane matter which each one must in the course of time suffer. The secular diminution of gravity would be more rapid according to this supposition than according to the former, but still might be made as slow as we please by pushing far enough the fundamental assumptions of very small diameters for the cage-bars of the mundane atoms, very great density for their substance, and very small volume and mass, and very great velocity for the ultramundane corpuscules.

The object of the present note is to remark that (even although we were to admit a gradual fading away of gravity, if slow enough), we are forbidden by the modern physical theory of the conservation of energy to assume inelasticity, or anything short of perfect elasticity, in the ultimate molecules, whether of ultramundane or of mundane matter; and, at the same time, to point out that the assumption of diminished exit velocity of ultramundane corpuscules, essential to Le Sage's theory, may be explained for perfectly elastic atoms, consistently both with modern thermodynamics, and with perennial gravity.

If the gravific corpuscules leave the earth or Jupiter with less energy than they had before collision, their effect must be to continually elevate the temperature throughout the whole mass. The energy which must be attributed to the gravific corpuscules is so enormously great, that this elevation of temperature would be sufficient to melt and evaporate any solid, great or small, in a fraction of a second of time. Hence, though outward-bound corpuscules must travel with less velocity, they must carry away the same energy with them as they brought. Suppose, now, the whole energy of the corpuscules approaching a planet to consist of trans-

latory motion: a portion of the energy of each corpuscule which has suffered collision must be supposed to be converted by the collision into vibrations, or vibrations and rotations. To simplify ideas, suppose for a moment the particles to be perfectly smooth elastic globules. Then collision could not generate any rotatory motion; but if the cage-atoms constituting mundane matter be each of them, as we must suppose it to be, of enormously great mass in comparison with one of the ultramundane globules, and if the substance of the latter, though perfectly elastic, be much less rigid than that of the former, each globule that strikes one of the cage-bars must (Thomson & Tait's "Natural Philosophy, § 301), come away with diminished velocity of translation, but with the corresponding deficiency of energy altogether converted into vibration of its own mass. Thus the condition required by Le Sage's theory is fulfilled without violating modern thermo-dynamics; and, according to Le Sage, we might be satisfied not to inquire what becomes of those ultramundane corpuscules which have been in collision either with the cage-bars of mundane matter or with one another; for at present, and during ages to come, these would be merely an inconsiderable minority, the great majority being still fresh with original gravific energy unimpaired by collision. Without entering on the purely metaphysical question,—Is any such supposition satisfactory? I wish to point out how gravific energy may be naturally restored to corpuscules in which it has been impaired by collision.

Clausius has introduced into the kinetic theory of gases the very important consideration of vibrational and rotational energy. He has shown that a multitude of elastic corpuscules moving through void, and occasionally striking one another, must, on the average, have a constant proportion of their whole energy in the form of vibrations and rotations, the other part being purely translational. Even for the simplest case,—that, namely, of smooth elastic globes,—no one has yet calculated by abstract dynamics the ultimate average ratio of the vibrational and rotational, to the translational energy. But Clausius has shown how to deduce it for the corpuscules of any particular gas from the experimental determination of the ratio of its specific heat pressure constant, to its specific heat volume constant.* He found that

* Maxwell's "Elementary Treatise on Heat," chap. xxii. Longman, 1871.

$$\beta = \frac{2}{3}\frac{1}{\gamma - 1},$$

if γ be the ratio of the specific heats, and β the ratio of the whole energy to the translational part of it. For air, the value of γ found by experiment, is 1·408, which makes $\beta = 1{\cdot}634$. For steam, Maxwell says, on the authority of Rankine, β "may be as much as 2·19, but this is very uncertain." If the molecules of gases are admitted to be elastic corpuscules, the validity of Clausius' principle is undeniable; and it is obvious that the value of the ratio β must depend upon the shape of each molecule, and on the distribution of elastic rigidity through it, if its substance is not homogeneous. Farther, it is clear that the value of β for a set of equal and similar corpuscules will not be the same after collision with molecules different from them in form or in elastic rigidity, as after collision with molecules only of their own kind. All that is necessary to complete Le Sage's theory of gravity in accordance with modern science, is to assume that the ratio of the whole energy of the corpuscules to the translational part of their energy is greater, on the average, after collisions with mundane matter than after inter-collisions of only ultramundane corpuscules. This supposition is neither more nor less questionable than that of Clausius for gases which is now admitted as one of the generally recognised truths of science. The corpuscular theory of gravity is no more difficult in allowance of its fundamental assumptions than the kinetic theory of gases as at present received; and it is more complete, inasmuch as, from fundamental assumptions of an extremely simple character, it explains all the known phenomena of its subject, which cannot be said of the kinetic theory of gases so far as it has hitherto advanced.

Postscript, April 1872.

In the preceding statement I inadvertently omitted to remark that if the constituent atoms are aeolotropic in respect to permeability, crystals would generally have different permeabilities in different directions, and would therefore have different weights according to the direction of their axes relatively to the direction of gravity. No such difference has been discovered, and it is

certain that if there is any it is extremely small. Hence, the constituent atoms, if aeolotropic as to permeability, must be so, but to an exceedingly small degree. Le Sage's second fundamental assumption given above, under the title "*Constitution of* "*Heavy Bodies*," implies sensibly equal permeability in all directions, even in an aeolotropic structure, unless much greater than Jupiter, provided that the atoms are isotropic as to permeability.

A body having different permeabilities in different directions would, if of manageable dimensions, give us a means for drawing energy from the inexhaustible store laid up in the ultramundane corpuscles, thus:—First, turn the body into a position of minimum weight; Secondly, lift it through any height; Thirdly, turn it into a position of maximum weight; Fourthly, let it down to its primitive level. It is easily seen that the first and third of those operations are performed without the expenditure of work; and, on the whole, work is done by gravity in operations 2 and 4. In the corresponding set of operations performed upon a moveable body in the neighbourhood of a fixed magnet, as much work is required for operations 1 and 3 as is gained in operations 2 and 4; the magnetisation of the moveable body being either intrinsic or inductive, or partly intrinsic and partly inductive, and the part of its aeolotropy (if any), which depends on inductive magnetisation, being due either to magne-crystallic quality of its substance, or to its shape.*

4. Note on Spherical Harmonics. By Professor Tait.

While engaged in some quaternion researches with reference to Spherical Harmonics, which I hope soon to lay before the Society, I was led to imagine that some of my results might produce a simplification of the ordinary modes of treating the subject. The following is the result of the attempt. It seems to make the cal-

* "Theory of magnetic induction in crystalline and non-crystalline sub-"stances."—*Phil. Mag.*, March 1851. "Forces experienced by inductively "magnetised ferro-magnetic and dia-magnetic non-crystalline substances." —*Phil. Mag.*, Oct. 1850. "Reciprocal action of dia-magnetic particles."—*Phil. Mag.*, Dec. 1855; all to be found in a collection of reprinted and newly written papers on electrostatics and magnetism, nearly ready for publication, (Macmillan, 1872).

culus somewhat more intelligible to the beginner than the methods employed by O'Brien and Murphy, whose works on the subject are usually read in this country. As I am not writing a treatise, but merely sketching a method, I shall run over the principal elementary propositions only.

1. Let

$$\frac{1}{\rho} = \frac{1}{(1-2h\mu+h^2)^{\frac{1}{2}}} = \Sigma_0^\infty h^i Q_i \ .$$

This is possible, if h be always taken less than 1; and, as μ is never beyond the limits ± 1, 1, Q_i, -1 are in order of magnitude, and the series is always convergent.

Hence we may differentiate, and we thus obtain

$$\frac{d}{d\mu}\frac{1}{\rho} = \frac{h}{\rho^3} = \Sigma_0^\infty h^i \frac{dQ_i}{d\mu},$$

and

$$\frac{d}{d\mu}\left((1-\mu^2)\frac{d\frac{1}{\rho}}{d\mu}\right) = \frac{d}{d\mu}\frac{(1-\mu^2)h}{\rho^3} = \rho^{-5}\left\{ -2\mu h\rho^2 + 3(1-\mu^2)h^2 \right\}$$

$$= \Sigma h^i \frac{d}{d\mu}\left((1-\mu^2)\frac{dQ_i}{d\mu}\right) \quad . \quad . \quad . \quad (1).$$

Also

$$h^2\frac{d}{dh}\frac{1}{\rho} = \frac{\mu h^2 - h^3}{\rho^3} = \Sigma . ih^{i+1} Q_i,$$

and

$$\frac{d}{dh}\left(h^2\frac{d}{dh}\frac{1}{\rho}\right) = \rho^{-5}\left\{ (2\mu h - 3h^2)\rho^2 + 3(\mu-h)^2 h^2 \right\}$$

$$= \Sigma . i(i+1) h^i Q_i \quad . \quad . \quad . \quad (2).$$

The sum of the multipliers of ρ^{-5} in (1) and (2) is obviously zero. Thus we have the equation for Q_i

$$i(i+1)Q_i + \frac{d}{d\mu}\left((1-\mu^2)\frac{dQ_i}{d\mu}\right) = 0 \quad . \quad (3).$$

2. From this equation, by differentiation $s-1$ times with respect to μ, we have

$$i(i+1)\frac{d^{s-1}Q_i}{d\mu^{s-1}} + (1-\mu^2)\frac{d^{s+1}Q_i}{d\mu^{s+1}} - 2s\mu\frac{d^sQ_i}{d\mu^s} - s(s-1)\frac{d^{s-1}Q_i}{d\mu^{s-1}} = 0,$$

or,

$$\Big(i(i+1)-s(s-1)\Big)(1-\mu^2)^{s-1}\frac{d^{s-1}Q_i}{d\mu^{s-1}}+\frac{d}{d\mu}\left((1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\right)=0 \quad (4).$$

3. Let Q_j be any one of the values of Q above defined, then

$$\int(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^sQ_j}{d\mu^s}d\mu=(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^{s-1}Q_j}{d\mu^{s-1}}-\int d\mu\frac{d^{s-1}Q_j}{d\mu^{s-1}}\frac{d}{d\mu}\left((1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\right)$$

$$=(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^{s-1}Q_j}{d\mu^{s-1}}+(i+s)(i-s+1)\int(1-\mu^2)^{s-1}\frac{d^{s-1}Q_i}{d\mu^{s-1}}\frac{d^{s-1}Q_j}{d\mu^{s-1}}d\mu.$$

Hence, integrating between the limits ∓ 1 of μ, we have

$$\int_{+1}^{-1}(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^sQ_j}{d\mu^s}d\mu=(i+s)(i-s+1)\int_{+1}^{-1}(1-\mu^2)^{s-1}\frac{d^{s-1}Q_i}{d\mu^{s-1}}\frac{d^{s-1}Q_j}{d\mu^{s-1}}d\mu \quad (5).$$

Applying the reduction s times, we evidently obtain

$$\int_{+1}^{-1}(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^sQ_j}{d\mu^s}d\mu=\frac{\lfloor i+s}{\lfloor i-s}\int_{+1}^{-1}Q_iQ_j\,d\mu \quad (6).$$

4. To find the value of the integral on the right, note that Q_iQ_j is the co-efficient of $h^ih'^j$ in the expansion of

$$\frac{1}{(1-2\mu h+h^2)^{\frac{1}{2}}(1-2\mu h'+h'^2)^{\frac{1}{2}}}$$

Now

$$\int_{+1}^{-1}\frac{d\mu}{\sqrt{(1+h^2-2h\mu)(1+h'^2-2h'\mu)}}$$

$$=\frac{1}{\sqrt{hh'}}\log.\frac{\sqrt{\frac{1+h^2}{2h}-1}+\sqrt{\frac{1+h'^2}{2h'}-1}}{\sqrt{\frac{1+h^2}{2h}+1}+\sqrt{\frac{1+h'^2}{2h'}+1}}$$

$$=\frac{1}{\sqrt{hh'}}\log.\frac{\sqrt{h'}(1-h)+\sqrt{h}(1-h')}{\sqrt{h'}(1+h)+\sqrt{h}(1+h')}$$

$$=\frac{1}{\sqrt{hh'}}\log.\frac{1-\sqrt{hh'}}{1+\sqrt{hh'}}$$

$$= -2\,\Sigma_0^\infty \frac{(hh')^i}{2i+1}.$$

In this there is no term in which the powers of h and h' are different, hence we have

$$\int_{+1}^{-1} Q_i\, Q_j\, d\mu = 0 \quad . \quad . \quad . \quad (7).$$

in all cases unless $j = i$. In this special case we have

$$\int_{+1}^{-1} Q_i^2\, d\mu = \frac{2}{2i+1} \quad . \quad . \quad . \quad (8.)$$

Hence the left hand member of (6) vanishes unless $j = i$, and in that case we have

$$\int_{+1}^{-1} (1-\mu^2)^s \left(\frac{d^sQ_i}{d\mu^s}\right)^2 d\mu = \frac{2}{2i+1}\,\frac{\underline{|i+s}}{\underline{|i-s}}. \quad . \quad (9).$$

We might have proved (7) from (6) by exchanging i and j, and showing that unless $i = j$, we cannot have

$$\frac{\underline{|i+s}}{\underline{|i-s}} = \frac{\underline{|j+s}}{\underline{|j-s}}.$$

5. The equation (3), which is satisfied by Q_i, is a mere particular case of the general equation of surface harmonics—

$$i(i+1)\, S_i + \frac{1}{1-\mu^2}\,\frac{d^2S_i}{d\varphi^2} + \frac{d}{d\mu}\left((1-\mu^2)\,\frac{dS_i}{d\mu}\right) = 0 \quad (10).$$

which may be obtained by putting $V_i = S_i$ in the ordinary equation of Laplace—

$$r\,\frac{d^2(rV_i)}{dr^2} + \frac{1}{1-\mu^2}\,\frac{d^2V_i}{d\varphi^2} + \frac{d}{d\mu}\left((1-\mu^2)\frac{dV_i}{d\mu}\right) = 0,$$

after differentiating the first term. That differentiation gives, in fact,

$$r\,\frac{d}{dr}\left(V_i + r\,\frac{dV_i}{dr}\right) = (i+1)\, r\,\frac{dV_i}{dr} = i(i+1)\, V_i.$$

From equation (10) we may prove, as usual, by multiplying by S_j and integrating over the unit sphere, that

$$i(i+1)\int d\sigma S_i S_j = j(j+1)\int d\sigma S_i S_j\,,$$

the expression for either being symmetrical in i and j, so that the integral vanishes unless $i=j$: or, if negative values be admitted, unless $i+j+1=0$.

6. We must now express S_i in terms of φ and Q_i. Let, then,

$$S_i = \Sigma_0 \; A_s \cos.(s\varphi + a_s)\, \Theta_i^{(s)} \quad . \quad . \quad . \quad (11).$$

where A_s, a_s are virtually $2i+1$ arbitrary constants. Substituting this value in (10), and supposing all the coefficients A to vanish except A_s, we have

$$\left(i(i+1) - \frac{s^2}{1-\mu^2}\right)\Theta_i^{(s)} + \frac{d}{d\mu}\left((1-\mu^2)\frac{d\Theta_i^{(s)}}{d\mu}\right) = 0 \;. \quad (12).$$

This equation is materially simplified by assuming (as is suggested by (6) and (9))

$$\Theta_i^{(s)} = (1-\mu^2)^{\frac{s}{2}}\,\theta_s \,. \quad . \quad . \quad . \quad (13),$$

for with this substitution it becomes, by a process the same as that of section 2 above,

$$\Big(i(i+1) - s(s+1)\Big)(1-\mu^2)^s\theta_s + \frac{d}{d\mu}\left((1-\mu^2)^{s+1}\frac{d\theta_s}{d\mu}\right) = 0\,.$$

But, by (4), putting $s+1$ for s,

$$\Big(i(i+1) - s(s+1)\Big)(1-\mu^2)^s\frac{d^sQ_i}{d\mu} + \frac{d}{d\mu}\left((1-\mu^2)^{s+1}\frac{d^{s+1}Q_i}{d\mu^{s+1}}\right) = 0\,.$$

Comparing these equations, and remembering that all the permissible arbitrary constants have already been introduced into the solution of (10), we have

$$\theta_s = \frac{d^sQ_i}{d\mu^s}\,.$$

Hence, finally,

$$S_i = \Sigma_0^i \; A_s \cos.(s\varphi + a_s)(1-\mu^2)^{\frac{s}{2}}\frac{d^sQ_i}{d\mu^s} \quad . \quad . \quad (14.)$$

7. We may now easily find the value of

$$\int S_i S_j \, d\sigma$$

taken over the whole spherical surface. For

$$\int (\qquad) \, d\sigma = \int_0^{2\pi} \int_{+1}^{-1} (\quad) d\phi \, d\mu :$$

and

$$\int_0^{2\pi} d\phi \cos.(s\phi + a_s) \cos.(s'\phi + a_{s'})$$

vanishes unless s and s' be equal, in which case its value is π. Hence, attending to § 4, and to (14),

$$\left.\begin{aligned} &\int S\,S\,d\sigma = 0, \\ \text{and} \quad &\int S_i^2 \, d\sigma = \frac{2\pi}{2i+1} \Sigma_0^i \, A_s^2 \frac{\underline{|i+s}}{\underline{|i-s}} \end{aligned}\right\} \quad . \quad . \quad . \quad (15).$$

8. Another curious expression for $\Theta_i^{(s)}$ is given by (4). For that equation gives

$$(1-\mu^2)_s \frac{d^s Q_i}{d\mu^s} = -\Big(i(i+1) - s(s-1)\Big) \int (1-\mu^2)^{s-1} \frac{d^{s-1} Q_i}{d\mu^{s-1}} d\mu$$

$$= + \{i(i+1) - s(s-1)\}\{i(i+1) - (\overline{s-1})(\overline{s-2})\} \iint (1-\mu^2)^{s-2} \frac{d^{s-2} Q_i}{d\mu^{s-2}} d\mu^2$$

$$= (-)^s \frac{\underline{|i+s}}{\underline{|i-s}} \int^{(s)} Q_i \, d\mu^s \quad . \quad . \quad . \quad . \quad (16).$$

Hence

$$\Theta_i^{(s)} = (-)^s \frac{\underline{|i+s}}{\underline{|i-s}} (1-\mu^2)^{-\frac{s}{2}} \left(\int d\mu\right)^s Q_i \quad . \quad (17).$$

10. Let

$$\sqrt{1+2\mu h+h^2} = 1+hy \quad . \quad . \quad . \quad (19),$$

where y is a function of h and μ, never beyond the limits $+1$ and -1.

Then

$$\frac{h}{\sqrt{1+2\mu h+h^2}} = h\frac{dy}{d\mu}.$$

But the first equation gives, at sight,

$$y = \mu + h\frac{1-y^2}{2} \quad . \quad . \quad . \quad (20),$$

whence,

$$y=\mu+h\frac{1-\mu^2}{2}+\frac{h^2}{1\cdot 2}\frac{d}{d\mu}\left(\frac{1-\mu^2}{2}\right)^2+\&c.,$$

and therefore,

$$\frac{1}{\sqrt{1+2\mu h+h^2}}=\frac{dy}{d\mu}=1+h\frac{d}{d\mu}\left(\frac{1-\mu^2}{2}\right)+\frac{h^2}{1\cdot 2}\frac{d^2}{d\mu^2}\left(\frac{1-\mu^2}{2}\right)^2+\&c.,$$

which shows that

$$Q_i=(-)^i\left(\frac{d}{d\mu}\right)^i\left(\frac{1-\mu^2}{2}\right)^i \quad . \quad . \quad (21),$$

and suggests obvious simplifications of preceding results, *e.g.*,

$$\theta_s=(-)^i\left(\frac{d}{d\mu}\right)^{i+s}\left(\frac{1-\mu^2}{2}\right)^i=(\text{by § 8})\,(-)^{i+s}(1-\mu^2)^{-s}\frac{|\underline{i+s}}{|\underline{i-s}}\left(\frac{d}{d\mu}\right)^{i-s}\left(\frac{1-\mu^2}{2}\right)^i,$$

&c., &c.,

11. The complete integral of

$$i(i+1)Q_i+\frac{d}{d\mu}\left((1-\mu^2)\frac{dQ_i}{d\mu}\right)=0 \quad . \quad (3)$$

may easily be found, since a particular integral is known. Let it be MQ_i, where M is a function of μ. Then (3) gives at once

$$\left(-2\mu Q_i+2(1-\mu^2)\frac{dQ_i}{d\mu}\right)\frac{dM}{d\mu}+(1-\mu^2)Q_i\frac{d^2M}{d\mu^2}=0,$$

or,

$$\frac{-2\mu}{1-\mu^2}+\frac{2}{Q_i}\frac{dQ_i}{d\mu}+\frac{1}{\frac{dM}{d\mu}}\frac{d^2M}{d\mu^2}=0,$$

whence

$$\frac{dM}{d\mu}=\frac{C}{(1-\mu^2)Q_i^2}.$$

Thus the complete integral is

$$CQ_i\int\frac{d\mu}{(1-\mu^2)Q_i^2} \quad . \quad . \quad . \quad (22).$$

12. Let us now suppose

$$S_i=P_iQ_i \quad . \quad . \quad . \quad (23),$$

where Q_i is as in § 1, and P_i is a function of μ and φ. The equation (10) becomes successively

$$\frac{d}{d\mu}\left((1-\mu^2)\frac{d\,(P_iQ_i)}{d\mu}\right)+\frac{1}{1-\mu^2}\frac{d^2(P_iQ_i)}{d\varphi^2}+i(i+1)\,P_iQ_i=0,$$

$$-2\mu\,Q_i\frac{dP_i}{d\mu}+(1-\mu^2)\left(2\frac{dP_i}{d\mu}\frac{dQ_i}{d\mu}+Q_i\frac{d^2P}{d\mu^2}\right)+\frac{Q_i}{1-\mu^2}\frac{d^2P_i}{d\varphi^2}=0,$$

$$(1-\mu^2)Q_i\frac{dP_i}{d\mu}\left(-\frac{2\mu}{1-\mu^2}+\frac{2}{Q_i}\frac{dQ_i}{d\mu}+\frac{1}{\frac{dP_i}{d\mu}}\frac{d^2P_i}{d\mu^2}\right)+\frac{Q_i}{1-\mu^2}\frac{d^2P_i}{d\varphi^2}=0,$$

$$(1-\mu^2)Q_i\,\frac{dP_i}{d\mu}\,\frac{\frac{d}{d\mu}\left((1-\mu^2)\,Q_i^2\,\frac{dP_i}{d\mu}\right)}{(1-\mu^2)\,Q_i^2\,\frac{dP_i}{d\mu}}+\frac{Q_i}{1-\mu^2}\frac{d^2P_i}{d\varphi^2}=0,$$

and, finally,

$$(1-\mu^2)\,Q_i^2\,\frac{d}{d\mu}\left((1-\mu^2)\,Q_i^2\,\frac{dP_i}{d\mu}\right)+Q_i^4\frac{d^2P_i}{d\varphi^2}=0.$$

If we put, for a moment,

$$\frac{d\mu}{(1-\mu^2)Q_i^2}=d\nu \quad \text{(which has a real meaning, see § 11),}$$

and suppose Q_i to be expressed in terms of ν instead of μ, calling it q_i, the equation may be written

$$\frac{d^2P_i}{d\nu^2}+q_i^4\,\frac{d^2P_i}{d\varphi^2}=0 \quad . \quad . \quad . \quad (24).$$

Hence it appears at once that P_i cannot contain φ except in the form of factors, such as cos. $s\varphi$, sin. $s\varphi$, in the several terms of which (as an integral of a linear equation) it must be composed. Hence, as before,

$$P_i=\Sigma_0^i\,A_s\,\overset{(s)}{\Theta_i}\,\cos.(s\varphi+a),$$

and, keeping to one value of s,

$$\frac{d^2\Theta_i^{(s)}}{d\nu^2}-\quad s^2q_i^4=0.$$

5. Laboratory Notes: On Thermo-Electricity. By Professor Tait.

For some time back I have been endeavouring to prove, by experiment, through great ranges of temperature, the result announced by me in December last, viz., that the electro-motive force of a thermo-electric circuit is in general, unless the temperature be very high, a parabolic function of the absolute temperature of either junction, that of the other being maintained constant.

For moderate ranges of temperature the experiment presents little difficulty; but, when mercurial thermometers cannot be employed, a modification of the experimental method must be made. I have employed in succession several such modifications, of which the following are the chief:—

The simplest of all is to dispense altogether with thermometers, and to employ two thermo-electric circuits, whose hot and whose cold junctions are immersed in the same vessels; and to plot the curve whose abscissæ and ordinates are simultaneous readings of the electro-motive forces in the two circuits. In every case I have tried the curve thus obtained is almost accurately a parabola, most of the few deviations yet observed being in the case of silver and other metals at temperatures not very much below their melting points—under circumstances, in fact, in which we should naturally expect that the law would no longer hold. There are, also, cases in which the whole electro-motive force is so small, even for very large differences of temperature, that very much more delicate apparatus would be required for their proper investigation. And there are cases in which the neutral point is so far off that for moderate ranges of temperature the curves obtained are sensibly straight lines. I intend to examine these cases with care—the former by using more delicate galvanometers; the latter, by employing metals which are practically infusible. The difficulty of obtaining wires of such metals has been the chief one I have had to face.

If we assume the experimental curve to be a parabola, then it is easily seen (*Proc.* May 29, 1871) that in each circuit the electromotive force must be a parabolic function of some function of the absolute temperatures of the junctions. And, as in the iron-silver,

iron-zinc, iron-copper, iron-cadmium, &c., circuits, this function has been proved to be simply the absolute temperature itself (at least, within the range of mercury thermometers), it is probable that such is the general law, at least for ranges of temperature short of those which materially alter the molecular structure of the metals employed.

The second method consisted in employing two pairs of circuits, all four hot junctions being in the same heated substance, and all four cold junctions kept at a common temperature. The members of each pair acted on a differential galvanometer (as explained in *Proc.* Dec.19, 1870) in such a way as to eliminate the term containing the square of the absolute temperature. In this case the readings of the galvanometers should be simply proportional to one another, and likewise to the differences of absolute temperature of the junctions. The method is exact in theory, but by no means easy in practice, especially with the very limited number of metals capable of resisting a high temperature which I could manage to obtain. That a very exact and useful thermometric arrangement can be made on this principle admits of no doubt, when we examine the results of the experiments.

The third method consisted in assuming the parabolic law, and the following consequence of it which follows directly by the use of Thomson's general formulæ. These may easily be reproduced as follows:—Suppose a sliding ring or clip to be passed round the wires, so as to press together points of the wires which are at the same temperature, t. Its effects are known by experiment to be nil, whatever be its material. Let it be slid along so that the temperature of what is now effectively the hot junction becomes $t + \delta t$, then the two laws of thermodynamics give, respectively,

$$\delta E = J\Big(\delta \Pi + (\sigma_1 - \sigma_2)\, \delta t\Big),$$

and

$$0 = \delta \frac{\Pi}{t} + \frac{\sigma_1 - \sigma_2}{t}\, \delta t .$$

Here E is the electromotive force, Π the Peltier effect at a junction at temperature t, and σ_1, σ_2, are the specific heats of electricity in the two metals.

Hence

$$\delta E = J\left(\delta\Pi - t\delta\frac{\Pi}{t}\right) = J\frac{\Pi}{t}\delta t\,.$$

Introducing the hypothesis, obtained from considerations of Dissipation of Energy, (*Proc.* Dec. 19, 1870) that

$$\sigma_1 = k_a t,\quad \sigma_2 = k_b t,$$

we have

$$J\frac{\Pi}{t} = \frac{dE}{dt} = (k_a - k_b)\ (T_{ab} - t)\,,$$

where T_{ab} is the well-known "neutral point."

Also

$$E = (k_a - k_b)\ (t - t_1)\left(T_{ab} - \frac{t + t_1}{2}\right),$$

since it vanishes for $t = t_1$, the temperature of the cold junction. Now, if the neutral point be between such limits as 0° C. and 300° C., the exact determination of it is an easy matter; and this exact knowledge of it greatly facilitates the determination of $\frac{dE}{dt}$, which cannot be *very* accurately found by drawing a tangent to the plotted curve. For if one junction be at t, the other at T_{ab}, we have

$$E_T = \tfrac{1}{2}(k_a - k_b)\ (T_{ab} - t)^2$$

E_T and $T_{ab} - t$ are easily measured on the experimental curve, and thus $k_a - k_b$ is found. The following values have thus been (roughly) calculated from observations. Where the neutral point was not reached, it is put in brackets. The unit for $k_a - k_b$ is 3 or 4 *per cent.* less than $\frac{2}{10^5}$ of the electromotive force of a good Grove's cell.

	T	$k_a - k_b$		T	$k_a - k_b$
Fe – Cu (bad)	265 C.	– 0·00147	Fe – Al	(387) C.	– 0·00105
,, – Cu (good)	260	– ·00145	,, – Arg.	(1357)	– ·00045
,, – Cd	159	– ·00209	Cu (bad) – Cd	– (23)	– ·00081
,, – Zn	199	– ·00189	,, – Zn	– (146)	– ·00048
,, – Ag	235	– ·00151	,, – Ag	– (687)	– ·00006
,, – Pb	(357)	– ·00112	,, (good) – Pb	– (213)	+ ·00016
,, – Brass	(318)	– ·00127	Pb – Cd	– (74)	– ·00096
,, – Pt	(519)	– ·00063	,, – Pd	– (188)	+ ·00080
,, – Sn	(416)	– ·00094	,, – Zn	– (78)	– ·00060
,, – Pd	(1908)	– ·00029	,, – Ag	– (262)	– ·00026

Now, it is an immediate consequence of the second law of thermodynamics that, as Peltier effects are reversible with the direction of the current, and are the *only* sensible thermal effects when a very feeble current passes through a thermo-electric circuit, all of whose parts are at one temperature, we must have

$$\Sigma \frac{\Pi}{t} = 0 ,$$

or, assuming the parabolic law,

$$\Sigma . (k_a - k_b) (T_{ab} - t) = 0 .$$

This holds for any number of separate materials in the conductor. As t is the same throughout, the terms involving it evidently vanish identically; but there remains the equation

$$\Sigma . (k_a - k_b) T_{ab} = 0,$$

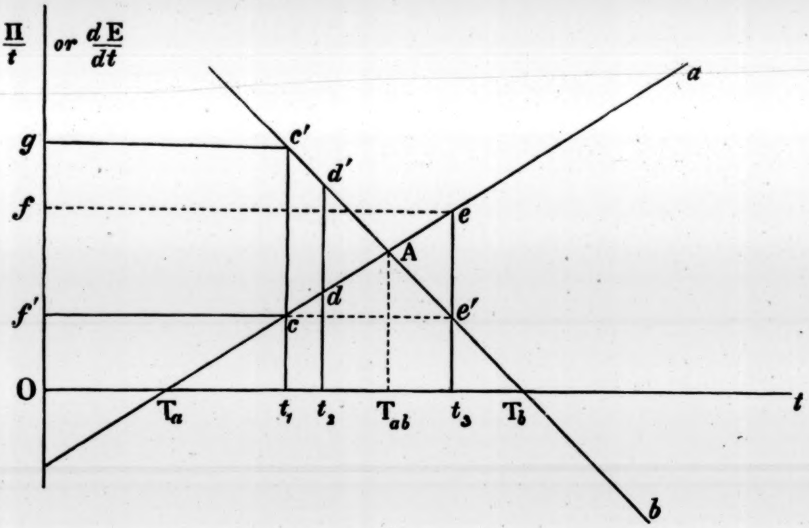
establishing a relation between the specific heats of electricity in a number of metals and the absolute temperatures of the neutral points of each junction of two of them. Other relations may be obtained by altering the order of the metals if there be more than three—but they are all virtually contained in the formula for three, which we write at full length,

$$(k_a - k_b) T_{ab} + (k_b - k_c) T_{bc} + (k_c - k_a) T_{ca} = 0.$$

From the direct experiments of Le Roux on "l'Effet Thomson," as he calls it, it appears that k is null in lead.* At all events, since Thomson showed that it has opposite signs in iron and copper, we may imagine a substance for which $k = 0$. We may now construct an improved "*Thermo-electric diagram*" to represent these relations numerically, employing the line for this substance as our axis of absolute temperatures; while the ordinates perpendicular to it give, for this substance employed with any other in a circuit of two metals, the values of $\frac{\Pi}{t}$, or $\frac{dE}{dt}$, or (what comes to the same thing) the electro-motive force of a circuit whose junctions are both very nearly at t, but have a small constant temperature difference. This quantity corresponds with what has been called the *thermo-electric power* of the circuit.

* Annales de Chimie, 1867, vol. x. p. 277.

The two oblique *straight* lines in the diagram belong to the metals a, b, respectively. The tangents of their inclination to the horizontal axis (the line of the supposed metal for which $k=0$) are k_a, k_b—and they cut it at the points T_a, T_b, where they are neutral to it; cutting one another at a point A whose abscissa is their own neutral point T_{ab}. The only change which would be introduced, by taking



as horizontal axis the line corresponding to a metal for which k does not vanish, would be a dislocation of the diagram, by a simple shear. This follows at once from the equation of one of the lines—

$$y = k_a (x - T_a).$$

The diagram gives the Peltier effect at the junction of a and b for any temperature t_1, by drawing the ordinate at t_1, and completing a rectangle $cc'gf'$ on the part intercepted, its opposite end being at absolute zero. The area of this rectangle is to be taken positively or negatively according as the corner corresponding to a is nearer to, or further from, the horizontal axis than that corresponding to b, the current being supposed to pass from a to b.

The electro-motive force in a circuit of the two metals, a and b, with its junctions at t_1 and t_2 respectively, is found by drawing ordinates at these temperatures, so as to cut off triangular spaces Acc', Add', whose vertices are at the neutral point. The difference

of the areas of these spaces, $cdd'c'$, is proportional to the electromotive force. When the higher temperature, t_3, is above the neutral point, the electromotive force is the difference of the areas Acc', Aee'. The case above mentioned, in which, by a differential galvanometer, we get rid of the terms in t^2, is obviously a process for making the curves of two separate complex arrangements into parallel straight lines.

In conclusion, I may give a few instances of the comparison of results of calculation of the neutral point of two metals from their observed neutral points, and differences of k, as regards iron, with calculation of the same neutral point from the portion of the curve (assumed to be a parabola) which expresses their electro-motive force within ranges of temperature where mercurial thermometers can be applied.

Thus with Fe, Cd, Pb, we have from the iron circuits $0{\cdot}00112 - 0{\cdot}00209 = -0{\cdot}00097$, while the direct experiment with Cd, Pb gave $-0{\cdot}00096$.

The neutral point, as calculated from the data for the iron circuits is $-69°$ C., while the calculation from direct experiment gives $-74°$ C.

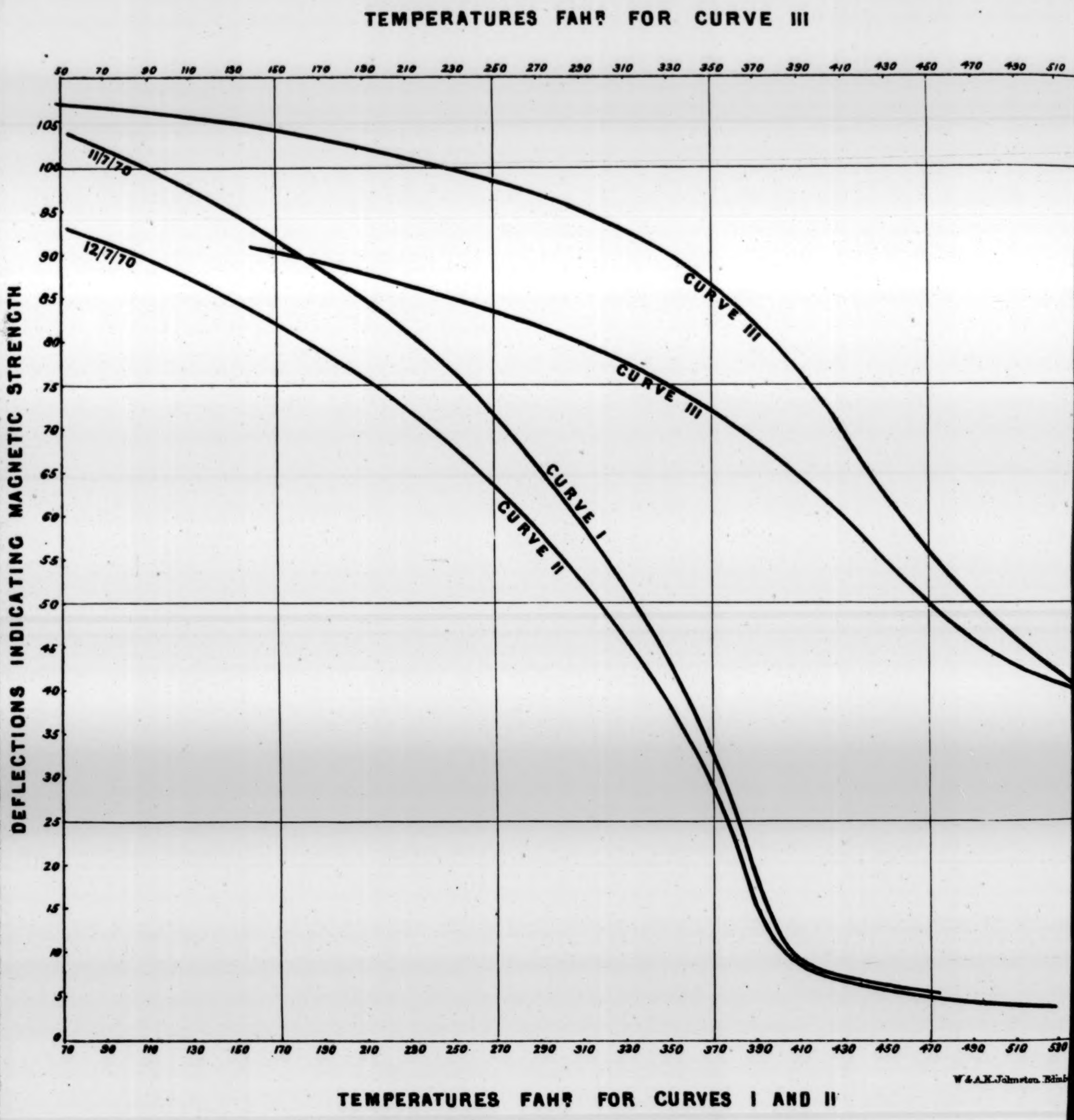
When the quantities to be found are very small, as for instance in the case Ag – Cu, we cannot expect to get a good approximation by introducing a third metal. In fact, introducing Fe we find indirectly $0{\cdot}00147 - 0{\cdot}00151 = -0{\cdot}00004$, while the direct determination gives $-0{\cdot}00006$.

Again with Zn and Cu, indirectly we get

	$-0{\cdot}00042$ and $-144°$ C.
Directly	$-0{\cdot}00048$ and $-146°$ C.

Several of the other groups give results as closely agreeing with one another as these, others are considerably out.

The numerical determinations above are founded entirely on a series of experiments made for me by Messrs J. Murray and R. M. Morrison. Mr W. Durham is at present engaged in determining the electromotive force of contact of wires of the same metal at different temperatures, with the view of inquiring into its relation to ordinary thermo-electric phenomena which appears to be suggested by some of the formulæ above given.



W & A.K. Johnston, Edinb

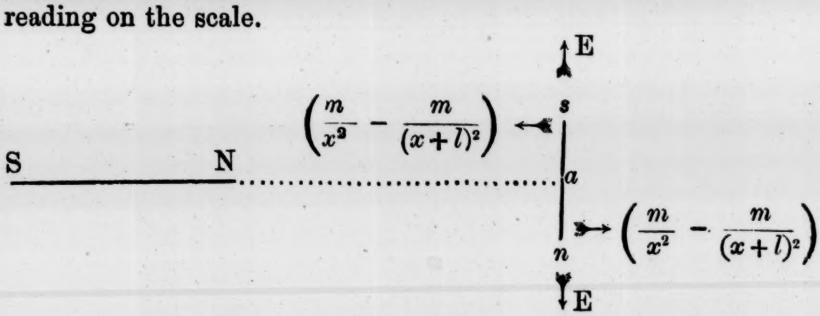
Monday, 15th January 1872.

PROFESSOR KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Relation of Magnetism to Temperature. (With a Plate.) By D. H. Marshall, Esq., M.A., Assistant to the Professor of Natural Philosophy. Communicated by Professor Tait.

The following was the arrangement adopted in these experiments:—A large magnet was put into a copper pot containing oil, which was heated up by a brass Bunsen, and its temperature determined by a mercurial thermometer immersed in it. The magnet was set magnetically east and west, and placed so as to act with equal force on the poles of a small magnet, whose centre was in the prolongation of its axis. This small magnet was cemented to the back of a small concave mirror, suspended by a single silk fibre, and placed in a glass case to guard it against currents of air. The deflections of the small magnet were measured exactly as in the reflecting galvanometer, and since from the nature of the arrangement, the absolute magnetism in the large magnet is directly as the tangent of the angle of deflection of the small one, its amount for any temperature was immediately measured by the reading on the scale.



N S, the poles of the fixed magnet, m its absolute magnetism. N $a=x$, S N$=l$. The couples indicated are those produced by the large magnet, and the earth's magnetism, E, on the small magnet.

For any deflection θ, if the length of the small magnet be negligible compared with x, we have

$$\mathrm{E} \sin. \theta = m \left(\frac{1}{x^2} - \frac{1}{(x+l)^2} \right) \cos. \theta \quad \therefore \; m \propto \tan. \theta .$$

[This simple formula holds, of course, however complex be the distribution of magnetism in the large magnet, provided the *relative* intensities of magnetization at different parts, and their directions, remain unchanged by heating.]

Disturbances were experienced in the form of thermo-electric currents in the pot and brass ring supporting it (these acted against one another), but their effects were rendered insignificant by removing the flame, and allowing the whole to come to a uniform temperature before reading. The direction of these currents, and therefore that of the disturbance to which they gave rise, could be reversed by changing the position of the flame relatively to the pot; but a smaller disturbance of a more unaccountable nature presented itself during the heating of the pot, which did not depend on the position of the flame, and could not be got rid of. This latter disturbance, which increased with the temperature, resulted in a gradual alteration of zero, and in consequence the deflections, corresponding at least to the higher temperatures in the curves and all the ordinates of the lower part of curve III., are somewhat less than they ought strictly to be.

Curves I., II., and the upper part of curve III., show how the absolute magnetism diminishes as the temperature of the magnet increases; the lower part of curve III. shows how the magnet regains its power when the temperature again falls, and it is seen at once from it that, when the magnet is allowed to cool after being heated, the deflection corresponding to a given temperature is less than that obtained at the same temperature when the magnet is being heated, thus indicating a loss of magnetic power, and the difference of the two deflections is greater the lower the temperature. It is principally on this account also that the curves I. and II. do not coincide, for the experiments were performed on successive days, and it was found that that magnet took about two days after such heating to acquire its original power. The magnet used

in the experiments represented by curves I. and II. was not the same as the one used in that represented by curve III.; the latter was a thin, very hard steel magnet, the former thicker and softer, and it may be seen from the curves that the hard steel parted with its magnetism less readily than the soft.

From these experiments it follows also that $\frac{dm}{dt}$, or the rate of change of magnetism with temperature, is not constant for each temperature, but depends in some way or other upon the state of the magnet.

When the above experiment was repeated with an electro-magnet in the copper pot instead of a permanent magnet, it was found that while at a temperature of 500° F. the power of the permanent magnet is very much lessened, that of the electro-magnet, provided the intensity of the current remain constant, is unaltered.

2. Note on a Singular Property of the Retina. By Professor Tait.

While suffering some of the annoyances seemingly inseparable from re-vaccination at too advanced an age, I was led to the curious observation presently to be described. I was unable to sleep, except in "short and far between" dozes, from which I woke with a sudden start, my eyelids opening fully. I found by trial that this state of things became somewhat less intolerable when I lay on my back, with my head considerably elevated. In this position I directly faced a gas jet, burning not very brightly, placed close to a whitish wall, and surrounded by a ground glass shade, through which the flame could be prominently perceived. The portions of the wall surrounding the burner were moderately illuminated, and hyperbolic portions above and below somewhat more strongly. I observed, on waking, that the gas flame seemed for a second or two to be surrounded by a dark crimson ground, though itself apparently unchanged in colour. Gradually, after the lapse of, at the very utmost, a couple of seconds, everything resumed its normal appearance. As this phenomenon appeared not only to be worthy of observation in itself, but to furnish me with something definite to reflect upon, which is far the best alleviation of annoy-

ances similar to those from which I was suffering, I determined to watch it, transitory as it was, feeling assured that I should have many opportunities of observing it. After two nights' practice, I found myself getting dangerously skilful in reproducing it, and decided, somewhat reluctantly, that I must give it up. What I observed, however, has already been almost completely described as having been seen on the very first occasion. I endeavoured to prepare myself to note any possible difference of colour in the crimson field, as distinguished from mere difference of intensity of illumination, and I could perceive none. I also endeavoured to ascertain the nature of the transition from this state to the normal one, but this was so exceedingly rapid that I could form no conclusion, and I found that under the necessary circumstances of the observation, viz., as it could be made only at the instant of awakening, it was impossible for me to estimate, even approximately, the duration of the crimson appearance.

Several possible modes of explaining the phenomenon at once occurred to me. Of these, however, I shall mention but three, and give reasons for rejecting two of them, while not pretending to specify them in the order in which they occurred to me. It cannot be ascribed to any visual defects in my eyes, which are normal as to colour sensations, and very perfect optically. 1*st*, I imagined it might be due to light passing through the almost closed eyelid, or through a portion of the eyeball temporarily filled with blood. Besides feeling certain that my eyes were fully open, I had the additional argument against this explanation, that I could not reproduce the phenomenon by carefully and gradually closing them, and that I am not aware that an effusion of blood in any part of the eye could possibly disappear so rapidly. 2*d*, It might be due to diffraction either by my eyelashes or by small particles, whether on the cornea or in the transparent substances of the eye, coarse enough to produce nearly the same tint for some distance round the flame. This is negatived by several considerations, among which (in addition to those urged against the preceding explanation) it is only necessary to mention again the facts, that the colour is not one which can be produced by diffraction under such circumstances, and that it appeared to be the same on the more illuminated, as well as on the darker part of the field.

3*d*, I suggest, as a possible explanation, but one which is more specially in the province of the physiologist than of the natural philosopher, that the retina (or the nerve cells connected with it?) partakes of sleep with the other nerve cells, by which that phenomenon has been accounted for, and that on a sudden awakening, the portions connected with the lowest of the primary forms of colour are the first to come into action, the others coming into play somewhat later, and almost simultaneously. This would completely account for the peculiar crimson colour, and for its uniformity of tint over the whole field, excepting the gas flame itself, the comparative intensity of whose light may easily be supposed to have simultaneously aroused all the three sensations in the small portion of the retina on which it fell, though it is just possible that it also may have appeared crimson for an exceedingly short period. I am not aware of any experiments or observations having been made with reference to the subject of this note, and I hope to have no further opportunities of making them, at least in the way in which these were made, but the point is a curious one, and worthy of the careful attention of all who may be forced to consider it. Professor Clerk-Maxwell informs me that he and others have observed that the lowest of the three colour sensations is the first to evanesce with faintness of light, and that it has been asserted to be the most sluggish in responding to the sudden appearance of light. This, however, is not necessarily antagonistic to my explanation, but will rather, if my explanation be correct, tend to show a greater interval between the awakening of the red, and that of the other colour sensations than that above hinted at.

3. On the Operator $\varphi(\nabla)$. By Professor Tait.

(*Abstract.*)

By combining, as above, Hamilton's linear and vector-function with his celebrated vector square-root of the negative of Laplace's operator, an operator of great use in physical applications of mathematics is obtained. With the notation employed in the author's paper "On Green's and other Allied Theorems," *Trans. R.S.E.*

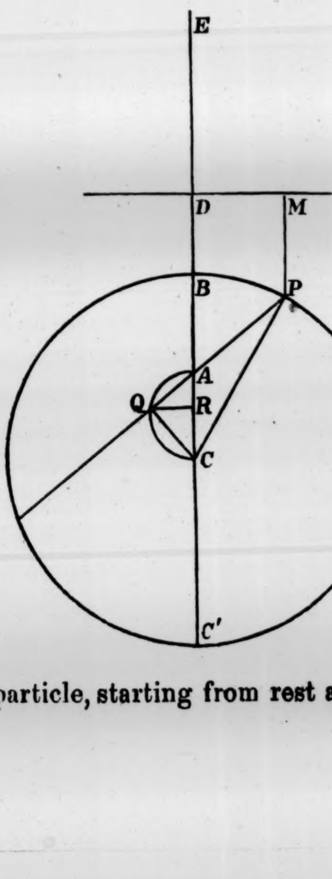
1870, § 17, it is shown to be generally expressible in the form of

$$\alpha_1 d_\alpha + \beta_1 d_\beta + \gamma_1 d_\gamma,$$

where α, β, γ, are any three unit vectors (not necessarily rectangular), and $\alpha_1, \beta_1, \gamma_1$, any three vectors whatever. The scalar and vector parts of the result of its operation on a vector-function, σ, of ρ are first considered—with various interpretations, especially as to distortions, condensations, &c., in a group of points—then it is exhibited in its applications to various questions; especially to Physical Strain, to Heat, and to Electricity. By making the constituents of ϕ variable, we have a means of Deformation specially applicable to problems such as that of Orthogonal Isothermal Surfaces.

4. Note on Pendulum Motion. By Professor Tait.

Mr Sang's papers in recent parts of the Transactions of the Society have reminded me of some geometrical constructions which are to a certain extent indicated in *Tait and Steele's Dynamics of a Particle* (1856). Some of these were suggested to me by a beautiful construction given (I believe by Clerk-Maxwell) in the *Cambridge and Dublin Math. Journal*, Feb. 1854, the others by a very simple process which occurred to me for the treatment of oscillations in cycloidal arcs. The former enables us easily to divide the arc of oscillation of a pendulum, or the whole circumference if the motion be continuous, into two, four, eight, &c., parts, which are described in equal times; also to solve by simple geometrical constructions problems such as the following:—Given any three points in a circle, find how it must be placed that a heavy particle, starting from rest at one of them, may take twice as long



to pass from the second to the third as it takes to pass from the first to the second. It suggested to me the following theorem, which really involves Mr Sang's results, but which appears to be considerably simpler in treatment, this being my sole reason for bringing it before the Society.

Let DM be a horizontal line, and let DA be taken equal to the tangent from D to the circle BPC′, whose centre C is vertically under D. Also let PAQ be any line through A, cutting in Q the semicircle on AC. Also make E the image of A in DM. Then if P move with velocity due to DM, Q moves with velocity due to the level of E; so that we have the means of comparing, arc for arc, two different continuous forms of pendulum motion, in one of which the rotation takes place in half the time of that in the other.

Let ω be a small increment of the circular measure of BAP, then

$$\text{arc at Q} = \text{CA} \,.\, \omega\,,\ \text{arc at P} = \frac{\text{AP} \,.\, \text{PC}}{\text{PQ}} \,.\, \omega\,.$$

Also,

$$\text{velocity at P} = \sqrt{2g \,.\, \text{PM}} = \sqrt{\frac{g}{\text{AC}}} \,.\, \text{AP}\,.$$

Hence,

$$\text{velocity at Q} = \frac{\text{CA} \,.\, \text{PQ}}{\text{AP} \,.\, \text{PC}} \sqrt{\frac{g}{\text{AC}}} \,.\, \text{AP} = \frac{\sqrt{g \,.\, \text{AC}}}{\text{PC}} \,.\, \text{PQ}\,.$$

But

$$\text{PQ} = \sqrt{\text{CP}^2 - \text{CQ}^2}$$

$$= \sqrt{\text{CP}^2 - \text{CR} \,.\, \text{CA}}\ \text{(where QR is horizontal)}$$

$$= \sqrt{\text{CA}} \sqrt{\frac{\text{CP}^2 - \text{CA}^2}{\text{CA}} + \text{AR}} = \sqrt{\text{CA} \,.\, \text{ER}}\,.$$

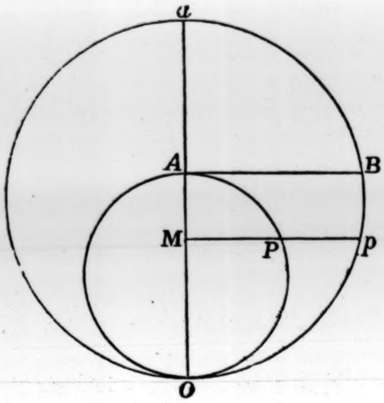
Hence,

$$\text{velocity at Q} = \frac{\text{AC}}{\text{PC}} \sqrt{g \,.\, \text{ER}}\,.$$

Thus Q moves with velocity due to the level of E, and constant acceleration

$$\frac{\text{AC}^2}{2\text{PC}^2} \,.\, g\,.$$

The second process referred to above gives at once the means of comparing continuous rotation with oscillation, as follows—



Let two circles touch one another at their lowest points—compare the arcual motions of points P and p, which are always in the same horizontal line. Draw the horizontal tangent AB. Then, if the line MPp be slightly displaced,

$$\frac{\text{Arc at P}}{\text{Arc at } p} = \frac{\text{AO}}{\text{MP}} \cdot \frac{\text{M}p}{a\text{O}} = \frac{\text{AO}}{a\text{O}}\sqrt{\frac{a\text{M}\,.\,\text{MO}}{\text{AM}\,.\,\text{MO}}} = \frac{\text{AO}}{a\text{O}}\sqrt{\frac{a\text{M}}{\text{AM}}}\,.$$

Thus, if P move, with velocity due to g and level a, continuously in its circle, p oscillates with velocity due to

$$g\,.\,\frac{a\text{O}^2}{\text{AO}^2} \text{ and level AB}\,.$$

Combining the two propositions, we are enabled to compare the times of oscillation in two different arcs of the same or of different circles.

Professor Cayley has pointed out to me that results of this kind depend upon one of the well-known fundamental transformations of elliptic functions. In fact, it is obvious that the squares of the sines of the quarter arcs of vibration which the combination of the above processes leads us to compare are (in the first figure),

$$\frac{\text{CA}}{\text{CE}} \text{ and } \frac{\text{C}'\text{B}}{\text{C}'\text{D}} \text{ respectively.}$$

Calling them $\frac{1}{k^2}$ and $\frac{1}{k_1^2}$, and putting DA $= a$, AC $= e$,

we have

$$\frac{1}{k^2} = \frac{e}{2a+e}\,,\ \frac{1}{k_1^2} = \frac{2\sqrt{2ae+e^2}}{a+e+\sqrt{2ae+e^2}}\,.$$

Hence

$$\frac{1}{k_1^{\,2}} = \frac{\frac{4}{k}}{1 + \frac{1}{k^2} + \frac{2}{k}},$$

or

$$\frac{1}{k_1} = \frac{2\sqrt{k}}{1+k}.$$

Lagrange's transformation is equivalent to

$$\sin.\,\varphi = \frac{2\sqrt{k}\sin.\,\theta}{1 + k\sin.^2\theta},$$

which, for limits 0 and $\sin.^{-1}\frac{1}{k}$ for θ, gives 0 and $\sin.^{-1}\frac{1}{k_1}$ for φ; and we thus have

$$\int_0^{\sin.^{-1}\frac{1}{k_1}} \frac{d\varphi}{\sqrt{\frac{1}{k_1^{\,2}} - \sin.^2\varphi}} = \frac{2k_1}{\sqrt{k}} \int_0^{\sin.^{-1}\frac{1}{k}} \frac{d\theta}{\sqrt{\frac{1}{k^2} - \sin.^2\theta}}.$$

whose application to the pendulum problem is obvious.

5. On the Decomposition of Forces externally applied to an Elastic Solid. By W. J. Macquorn Rankine, C.E., LL.D., F.R.SS. Lond. and Edin.

(*Abstract.*)

The principles set forth in this paper, though now (with the exception of the first theorem) published for the first time, were communicated to the French Academy of Sciences fifteen years ago, in a memoir entitled "de l'Equilibre intérieur d'un Corps solide, élastique, et homogène," and marked with the motto, "Obvia conspicimus, nubem pellente Mathesi," the receipt of which is acknowledged in the Comptes Rendus of the 6th April 1857 (vol. xliv. p. 706.)

The author quotes a theorem discovered by him, and previously published in the Philosophical Magazine for December 1855, called "the Principle of Isorrhopic Axes," viz., "Every self-

balanced system of forces applied to a connected system of points, is capable of resolution into three rectangular systems of parallel self-balanced forces applied to the same points."

Let X, &c., be the forces resolved parallel to any three orthogonal axes; find the six sums or integrals, ΣXx, ΣYy, ΣZz, $\Sigma Yz = \Sigma Zy$, $\Sigma Zx = \Sigma Xz$, $\Sigma Xy = \Sigma Yx$; these are called the "rhopimetric coefficients." Conceive the ellipsoid of whose equation these are the coefficients; then for the three axes of that ellipsoid (called the "isorrhopic axes") each of the last three coefficients is null; and the three systems of forces parallel respectively to those three axes are separately self-balanced.

The theorem may be extended to systems of moving masses by putting $X - m\frac{d^2x}{dt^2}$, &c., instead of X, &c. If for any system of forces, the last three rhopimetric coefficients are null, and the first three equal to each other, every direction has the properties of an isorrhopic axis. This, of course, includes the case in which all the coefficients are null; and in that case the system of forces is said to be "Arrhopic." The author shows that the six rhopimetric coefficients of a system of forces externally applied to an elastic solid, being divided by the volume of the solid, give the mean values throughout the solid of the six elementary stresses. Those are called the "Homalotatic stresses."

If we calculate from them the corresponding externally applied pressures, these may be called the "Homalotatic pressures."

Take away the homalotatic pressures from the actual system of externally applied pressures, and the residual pressures will be arrhopic; that is to say, their components parallel to any direction whatsoever will be separately self-balanced, and may have their straining effects on the solid separately determined; and hence, the axes to which those residual pressures are reduced may be arbitrarily chosen, with a view to convenience in the solution of problems.

The author then demonstrates that those problems respecting the distribution of stress in an elastic solid, in which the stresses are expressed by constants and by linear functions of the co-ordinates, are all capable of solution independently of the coefficients of elasticity of the substance.

6. On Geometric Mean Distance. By Professor Clerk Maxwell.

7. On a Singular Case of Rectification in Lines of the Fourth Order. By Edward Sang, Esq.

The class of curves resulting from the formula

$$x = a \,.\sin \theta \,,\; y = b \,.\sin 2\theta$$

are of considerable interest as occurring in various mechanical inquiries. When a straight wire, whose effective breadth and thickness are as two to one, is fixed at one end and made to vibrate, its free end describes a curve of which the general equation is

$$x = a \,.\sin (\theta + k) \;,\; y = b \,.\sin 2\theta \,,$$

in which k is constant for the particular variety of curve. When $k = \frac{\tau}{2}\pi$ the curve becomes a parabola, and when $k = o$, it takes the form above mentioned; these phases were exhibited by me in 1832. Again, when a system of toothed wheels is deduced from a straight rack, having a curve of sines for its outline, the points of contact describe a curve of this class, as is shown in my treatise on the teeth of wheels.

In attempting the rectification of these curves, we have to integrate an expression of the general form

$$dl = \{ a^2 .\cos \theta^2 + 4\,b^2 .(\cos 2\,\theta)^2\}^{\frac{1}{2}} d\theta \,,$$

and for this purpose have to expand the root in an interminate series, and then integrate each term, the result being unmanageable from its complexity. In one particular phase of the curve, however, the integration can be easily effected. The above general expression may be written

$$dl = \{ 16\,b^2 .\cos \theta^4 + (a^2 - 16\,b^2) \cos \theta^2 + 4\,b^2\}^{\frac{1}{2}} d\theta \,,$$

and we readily observe that if $a^2 = 32\,b^2$, that is, if $a = 4\,b\sqrt{2}$,

the quantity under the radical sign becomes a square, and in this case

$$d\,l = \{\,4\,b\,.\cos\theta^{3} + 2\,b\,\}\,d\,\theta$$
$$= \quad 2\,b\,\{\cos 2\,\theta + 2\,\}\,d\,\theta\,,$$

whence, on integrating, we at once obtain

$$l = b\,\{\,\sin ?\,\theta + 4\,\theta\,\} = y\,4\,b\,\theta\,.$$

The expression for the radius of curvature also takes a very simple form, it is

$$r = \frac{b}{\sqrt{2}}\,\frac{(\cos 2\,\theta + 2)^{3}}{\sin\theta}\,.$$

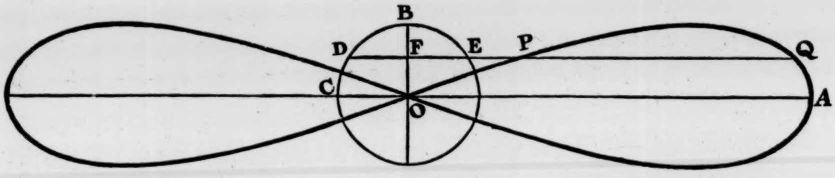
No other curve of this class, nor indeed any belonging to the more general formula

$$x = a\,.\sin\left(p\,\theta + k\right),\, y = b.\sin\left(q\,\theta\right),$$

seems to be susceptible of easy rectification.

These results may be exhibited geometrically thus:—Having drawn OA, OB in the directions of the length and breadth of the curve, and described round O a circle with the radius OB = OC = *b*, let OA be made equal to four times CB, and an hour-glass curve be constructed in the usual manner. Then, having assumed any arc CD to represent $b\,.\,2\,\theta$ and drawn DFQ parallel to OA, if FP be laid off equal to $a\,.\sin\theta$, P is a point in the curve, and the length from O to P is equal to the sum of OF, and twice the arc CD.

Hence it follows that the portion PQ of the curve, cut off by the line DQ, is just double of the circular arc DBE, cut off by the same line.



Hence it appears that the length of the quadrant OPQA of the curve is just equal to the circumference of the circle, or that the whole curve is equal in length to four times the circumference of the circle described with the radius OB.

The following Gentlemen were admitted Fellows of the Society:—

David Maclagan, Esq., C.A.
Major Rickard.
Dr John Sibbald.
Dr J. G. Fleming.
Rev. Andrew Tait, LL.D.
David Grieve, Esq.
The Right Rev. Bishop Cotterill.
George Barclay, Esq.

Monday, 29th January 1872.

The Hon. LORD NEAVES, Vice-President, in the Chair.

The following Communications were read:—

1. On the Wheeling of Birds. By Professor Fleeming Jenkin.

2. Notice of a New Family of the Echinodermata. By Professor Wyville Thomson, LL.D., F.R.SS.L. and E., F.L.S., F.G.S.

During the deep sea dredging expedition of H.M. ships 'Lightning' and 'Porcupine,' in the summers of 1868–69 and 1870, two or three nearly perfect specimens, and a number of fragments were procured of three species of regular echinideans, which were referred by the author to a new family, the Echinothuridæ, intermediate in their more essential characters between the Cidaridæ and the Diadematidæ.

In these urchins the test is circular and greatly depressed. The plates of the perisom are long and strap-shaped, and the interambulacral plates overlap one another regularly from the apical towards the oral poll, while the ambulacral plates overlap in a similar way in the opposite direction. The test is thus flexible. The plates of the ambulacral areæ are essentially within the interambulacral plates which over-lie them along their outer edges. The ambulacral pores are tri-geminal. arranged in wide arcs; the

two pairs of pores of each arc which are nearest the centre of the ambulacral area, pierce two small accessory plates intercalated between the ambulacral plates, while the outer pair passes through the ambulacral plate itself near its outer extremity. The tube-feet on the oral surface of the body are provided with terminal suckers, supported by calcareous rosettes, while those on the apical surface are conical and simple. The tube-feet on both surfaces have their walls supported by wide cribriform calcareous plates.

The peristome and the periproct are unusually large. The edge of the peristome is entire, without branchial notches, and the peristomial membrane is uniformly plated with twenty rows of imbricating scales, corresponding with the rows of plates of the corona, and the rows of ambulacral tube-feet are continued as in the Cidaridæ, over the peristome up to the edge of the mouth. The ovarial plates are unusually large; in some of the species they are broken up into several calcareous pieces. The ovarial apertures are very large, and are partly filled up with membrane.

The dental pyramid is wide and strong, but somewhat low on account of the depressed form of the test. The epiphyses of the tooth-sockets do not form closed arches as in the Echinidæ, and in this respect resemble those of *Cidaris* and *Diadema*. The teeth are simply grooved as in *Cidaris*. The spines are hollow and comparatively small, and the larger spines show a tendency to the spiral arrangement of projecting teeth which is so characteristic of the Diadematidæ. The Pedicellariæ are very remarkable in form, more nearly related, however, to those of the Diadematidæ than to any others. A strong fenestrated fascia traverses the body cavity vertically on either side of each ambulacral area, thus nearly cutting off the ambulacral from the inter-ambulacral region, and allowing only a small space for the coils of the intestine.

For this family, distinguished by the depressed corona of imbricated plates, the peristome covered with scales through which the rows of ambulacral double-pores are continued to the mouth, the absence of branchial notches in the peristomial border, the peculiar arrangement of the ambulacral pores, the heterogeneity of the tube-feet on the oral and apical surfaces, the absence of closed arches uniting the pairs of tooth-sockets, and the absence of longitudinal ridges within the simple grooved teeth, the term

Echinothuridæ was proposed, the fossil-genus *Echinothuria*, sagaciously described by the late S. P. Woodward, from an imperfect specimen from the upper chalk being taken as the type. The specimens procured were referred to two genera and three species.

In the genus *Phormosoma* the plates of the perisom only slightly overlap, and fit so closely as to form a complete calcareous casing without any membranous fenestræ. Although constructed essentially on the same plan, the apical and oral surfaces of the test differ from one another singularly in character, the oral surface being almost uniformly covered with large areolar depressions surrounding spine tubercles.

One species, *Phormosoma placenta*, n. sp., was dredged in deep water off the Butt of the Lews, and some fragments were met with in gravel from the Rockall Channel.

In the genus *Calveria*, the plates of both the ambulacral and inter-ambulacral areas form large expansions towards the middle line of the area, while the outer portions of the plates are narrow and strap-shaped, leaving large fenestræ filled up with membrane between plate and plate. The oral surface of the body does not differ markedly in character from the apical.

Two species of this genus were taken, *Calveria hystrix*, n. sp., with a strong perisom, of a nearly uniform rich claret colour, from deep water off the Butt of the Lews; and *Calveria fenestrata*, n. sp., more delicate, with wider spaces between the plates, the body of a greyish colour, rayed from the apical pole with bright chocolate.

It is very possible that the genus *Asthenosoma*, described by Professor Grube, may belong to this group, but the description of that form hitherto given is not sufficient for identification, as the points of structure on which the families of the Echinidea are distinguished from one another are not noticed. With this exception, the form which most nearly resembles them is *Astropyga*, which, however, is in every respect, except in habit, a true *Diadema*, with the peristomial margin deeply notched for external branchiæ, and all the other characters of the family.

3. On the Principles which regulate the Incidence of Taxes. By Professor Fleeming Jenkin.

It is well known that many taxes do not fall ultimately on the person from whom they are in the first instance levied. The merchant advances the duties imposed on goods, but the tax ultimately falls on the consumer. The problem of discovering the ultimate or true incidence of each tax is one of great importance, and of considerable complexity. The following paper contains an investigation of the methods by which this incidence may in some cases be experimentally determined, and of the principles regulating the incidence in all cases, these principles being stated in a mathematical form.

The author, in a paper published in Recess Studies, expressed the law of supply and demand by representing what may be termed the demand and supply functions, as curves. The ordinates parallel to the axis OX, fig. 1, were prices—the coordinates parallel to the axis OY were the supplies at each price, and the demand at each price for the respective curves—the market price is then indicated by the ordinate X of the point at which the curves intersect, this being the only price at which buyers and sellers are agreed as to the quantity to be transferred.

We might write the law algebraically as follows, calling y the quantity of goods in the market, at each price x, we have $y = \varphi x$; and calling y_1 the quantity of goods demanded at each price, we have $y_1 = \varphi_1 x$; the market price is determined by the equation $y = y_1$. There is, however, little or no advantage in adopting this algebraic form, because we cannot suppose that in any instance φx or $\varphi_1 x$ will be any tolerably simple algebraic function, whereas the curve for given goods might be determined experimentally by observing from year to year variations of quantities bought or quantities supplied at various prices.

Professor Jevons has since given a much more complex algebraic representation of the same law, which, however, reduces itself to the above simple form.

The graphic method may also be employed to indicate the advantage gained by each party in trade, and to show how it may be estimated in money. Let the two curves indicate the demand

and supply at each price for a certain kind of goods. If all sellers were of one mind, and were willing to supply all their goods at a given price x, and were quite determined to sell no goods below that price, the supply curve would be a mere straight line parallel to OX, and ending abruptly at the ordinate raised at x. Similarly, if all buyers were of one mind, and would only buy below a given price x, but were willing to buy all they want at that price, and no more at any lower price, the demand curve would be a line parallel to OX ending abruptly at the ordinate raised at x, and the price would be quite indeterminate. If the two lines overlapped, transactions might take place at any price between that at which the

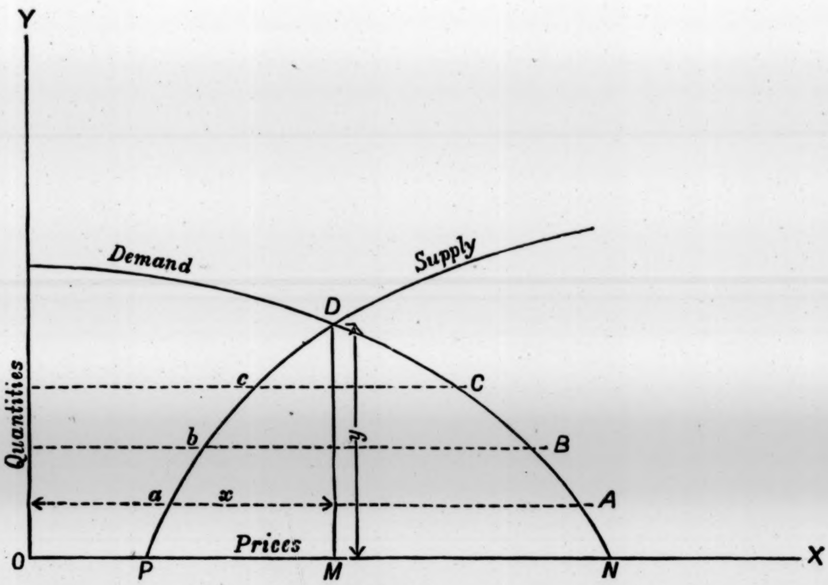


Fig. 1.

sellers were willing to sell and the buyers willing to buy; there would in this case be no market price. This case does not represent the true state of either buyers' or sellers' minds in any real large market. There are always a few holders who would only sell if the price were much higher than the market price,—these are the people who expect prices to rise; there are some who are just willing to sell at the market price, but who will not sell a penny below; and there are others, weak holders, who expect prices to fall, and these would really, if pushed to extremity, sell below the market price. This condition of things is represented by the supply curve in fig. 1.

Similarly, there are a few buyers who, if pushed to extremity, would buy some goods above market price; some also will just buy at market price; some will not buy unless the price is below market price. This is represented by the demand curve.

Now, I contend that when the market price is fixed, those traders who are perfectly indifferent whether they buy or sell at that price reap no benefit by the trade; but these will be few in number.

Looking at the demand curve, the ordinate X_A from the axis OY to A represents the value set on some of the goods by some buyers, but these buyers have got the goods for the sum represented by the ordinate $x = OM$; the difference between these two ordinates $X_A - x$ is the difference in price between what was given and what might have been given for a certain small quantity Δy of goods. Δy is therefore the benefit reaped by buyers from the purchase of the quantity Δy; and integrating the benefits derived from the sale of each successive quantity, we find the area MDCBAN represents the whole gain to buyers by the purchase of the quantity y of goods. Similarly, it is easy to show that the area MD*cba*P represents the gain to sellers by the same transaction; these areas represent the gain in money. Each product $\Delta y(x - X_A)$ being the product of a quantity by the gain in money per unit of quantity.

Thus the whole benefit to the two leading communities is represented by the sum of the two above named areas, and the repartition of the benefit between the two communities is perfectly definite.

Professor Jevons has used curves to integrate what he terms the utility gained by exchange in a manner analogous to the above; but utility, as he defines it, admits of no practical measurement, and he bases his curve, not on the varying estimates of value set by different individuals each on what he has or what he wants, but on the varying utility to each individual of each increment of goods. The above estimate of the gain due to trade, deduced from the demand and supply curves as originally drawn in my Recess Studies' article is, I believe, novel, and gives a numerical estimate in money of the value of any given trade, which might be approximately determined by observing the effect of a change of prices on the trade; the curves throughout their whole lengths could cer-

tainly not, in most cases, be determined by experiment, but statistics gathered through a few years would show approximately the steepness of each curve near the market price, and this is the most important information.

A steep supply curve and a horizontal demand curve indicate that the buyers reap the chief benefit of the *trade*. The sellers, if producers, may, however, be making important profits as capitalists and labourers.

A steep demand curve and a level supply curve indicate that the suppliers are chiefly benefited by the trade; the community or body which is most ready to abandon the trade if the price increases a little, benefits least by the trade.

When the traders are producers and consumers, the benefits estimated in this way as due to the *trade* are not the only benefits reaped by the community from the manufacture.

In this case, what is termed the supply curve depends on the cost of production of the article, including that interest on capital and that remuneration for skilled superintendence which is necessary to induce the producer to employ his capital and skill in that way. The cost of production increases generally with the quantity of the article produced, otherwise the supply curve would be a straight vertical line; but as a matter of fact, to produce an increase of production a rise of price is necessary, indicating that only a few men with little capital are content with a small rate of interest and small remuneration for their skill, but that to induce many men and much capital to be employed in the particular manufacture, a large rate of interest and considerable remuneration are required, hence the supply curve will be such as shown in fig. 2, where the price OP is that price or cost of production which is just sufficient to tempt a few producers to produce a little of the article.

Then if OP′ is the actual cost out of pocket required to produce a small quantity of an article, and if OP is the lowest cost at which any manufacturer can afford to produce it, the area P′D′DM represents the whole profit to the producing capitalist when the price is OM. The line D′P′ is not necessarily parallel to DP, nor vertical, the bare cost of production of the article generally increases as the quantity increases; and in that case D′P′ is not vertical. Again, the rate of interest required to tempt additional capital

into a particular field is not constant, but increases, hence P′D′ is steeper than PD. I see at present no means of experimentally ascertaining the gain reaped by producers represented by the area PDD′P′; it can be approximately estimated by considering the prevailing rate of interest in the producing community and the amount of capital required for the production of the unit of the article.

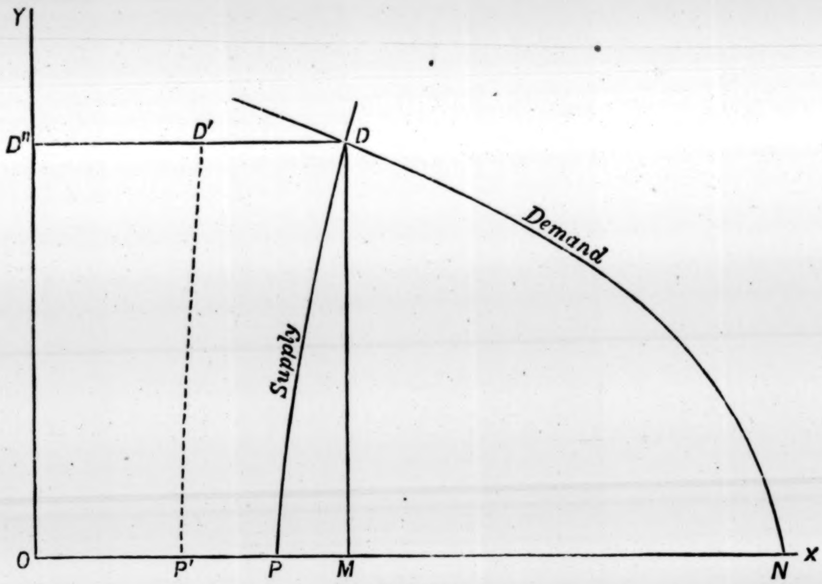


Fig. 2.

We see that the gain of a manufacturing capitalist may be divided into two parts—the profit as a trader, and the interest as a capitalist.

In safe trades, where there are few fluctuations in price, the former gain may perhaps be the most important; in more speculative trades the latter.

There is yet a third source of gain to the manufacturing community: the labourer who produces the goods earns his wages by the manufacture, and this is an advantage to him. In the diagram, the area OP′D′D″ represents the wages paid for labour alone. The length of the lines between OY and P′D′ represent the wages of labour per unit of goods, increasing as the quantity of goods required increases. This is lost to the community if the manufacture is stopped. Thus the whole sum paid by the consumer is the area OMDD″; and this is made up of three parts, one of which

is the profit to the trader, one the interest to the capitalist, and one the wages of the labourer; all these advantages are lost if the manufacture ceases.

The gain of the labourer does not resemble the profit of the trader, or the interest of the capitalist. The profit of the trader is the difference between his valuation of the goods and what he gets for them. If he does not sell his goods he still has his goods, he only loses the profit. Similarly, if the capitalist does not sell his capital, he still has his capital. Now, the area P′PDD′ represents the profit made by the capitalist on the particular employment of his capital, and this is all that he loses if unable to sell that capital; but the area OP′D′D″ represents the whole sum received by the labourers, not their profit. The profit of the labourer may perhaps be considered as the excess of wages which he earns in a particular trade, over that which would just tempt him to work rather than starve or go into the workhouse.

If the consumer purchases the article for simple unproductive consumption, then the loss to him is only represented by the area DMN. If, however, a community purchases goods, and consumes them productively, then, by the cessation of the trade, they in their turn lose the interest on the capital they employ, and the labourers of the community lose their wages; so that, in that case, the loss to the buyer, who cannot be classed as an immediate consumer, is made up of three parts, similar to those enumerated in the case of the seller.

Taxes on Trade.

Having distinguished between the three distinct advantages given by trade, I will now consider the incidence of a tax on trade, levied as a fixed sum per unit of goods, as one pound per ton, or one shilling per gross.

The effect of such a tax is to produce a constant difference between the price paid by the buyer and the price received by the seller. The market prices are determined in the diagram of the supply and demand curves, by the points between which a line parallel to OX, and equal in length to the tax, can be filled between the two curves.

Thus, if in figure 3, FN be the demand curve, and PE the supply curve, and if the length of the line CC′ be the amount of

the tax per unit of goods, then OM is the market price to the supplier, OM′ the market price to the buyer and the difference Mᴍ′ is equal to the tax.

The total amount raised by the tax from the transactions represented in the diagram, is measured by the area MCC′M′. The

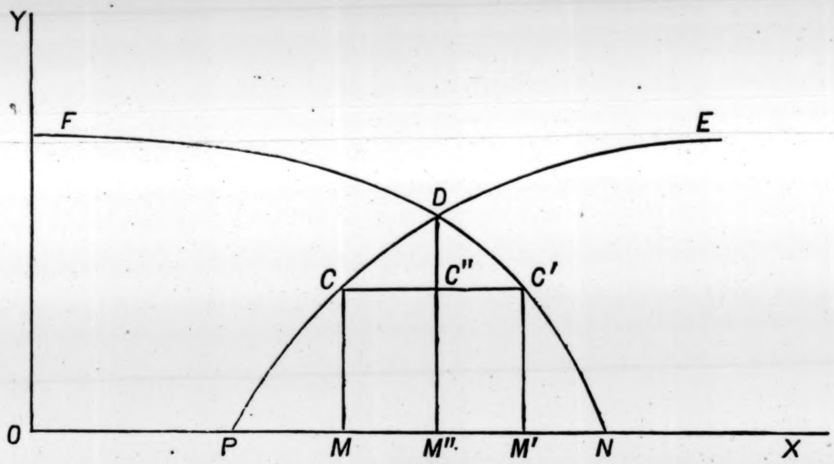


Fig. 3.

portion paid by the seller is measured by the area CC″M″M. The portion paid by the buyer is measured by the area C″C′M′M″. The whole loss entailed by the tax on the two communities is measured by the area MCDC′M′; the loss to the sellers is measured by the area CDM″M; the loss to the buyers by the area M″DC′M′; both buyers and sellers suffer a loss beyond the tax they pay. This excess of loss is represented by the area CC″D for the sellers, and C′C″D for the buyers.

If the tax be large, the line CC′ will approach the axis OX, the tax will be unproductive, and the area CC′D representing the excess of injury to the buyers and sellers will be large, compared with the produce of the tax. This fact is one justification of free trade.

There is a certain magnitude of tax which will produce the maximum revenue or value for the area MCC′M′. The ratio in which the tax falls, in one sense, on sellers and buyers is simply the ratio of the diminution of price obtained by the sellers to the increase of price paid by the buyers.

It is absolutely clear that this is the proportion in which the tax is actually *paid* by the two parties, although this may by no means

correspond to the relative suffering inflicted on the two parties, nor is it even the proportion in which the two parties lose by the loss of trade profit. The whole loss of either party is, as the diagram shows, always greater than the tax they pay. The relative total losses of the two communities as traders, are in proportion to the areas MCDM″ and M′C′DM″; and these areas might approximately, at least, be ascertained by experiments for this purpose, treating CD and C_1D as straight lines, we only require to know the quantity and price of the goods before the imposition of the tax, and the quantity and price afterwards.

Thus, if a tax of 2d. per pound were imposed on the trade in cotton between ourselves and America, if before the tax we imported 500 million lbs. at one shilling, and after the tax 300 million lbs. for which we paid 13½d., and the Americans received 11½d., the total loss to the two communities as traders would be $600 + 200 = 800$ million pennies, the produce of the tax 600 million pennies.

England would pay of the tax 450 million pennies. England's total loss would be 600 million pennies. America would pay of the tax 150 million pennies. America's total loss would be 200 million pennies. The incidence would be the same whichever government levied the tax.

It follows from the above principles, that if a holder sells unreservedly, trusting to the competition between the buyers to produce the market, the whole tax falls on the seller; the supply curve becomes a vertical straight line. If a buyer buys unreservedly, the whole tax falls on him; in this case the demand curve becomes a vertical straight line.

Thus, if sales by auction were subject to a tax *ad valorem* or otherwise, and if sales were quite unreserved, the number of transactions not being altered, the prices would be unaltered, but the sellers would only get the prices minus the tax.

This case does not practically arise, because, if auctions were really so taxed, although in each auction that occurred the sale might be unreserved, auctions would, as a whole, be checked; fewer people would put up their goods for sale in that way,—the prices would rise, the number of transactions would be diminished, and the tax would really be borne in part by the buyers and part by the sellers.

If the trade between two countries really consists in the exchange of goods, effected by the agency of money as a unit for expressing value, but not involving the actual transfer of coin, the above principles show the whole gain by the exchange to be the sum of two gains which each party would make by each trade if it alone existed.

If by duties one portion of the trade be extinguished or much diminished, both parties lose, but if the other portion of the trade remain uninjured, then, although there may be no exchange of commodities other than of goods for actual money, nevertheless the full gain on that which is untaxed remains intact. Thus, although the French may tax our goods, and so inflict a loss on themselves and on us, this is no reason for our inflicting an additional loss on the two communities by taxing the import of their goods.

House Rent.

I will next consider the effect of a tax on house rent.

Landlords are here the sellers, and tenants the buyers of what may be termed a commodity; not the house, but the loan of a house for a term of years—the tenant buys what might be called, by the extension of a suggestion of Professor Jevons, a *house-year* from his landlord.

The difference between the house and other commodities such as food or dress is, that the house remains, whereas they are consumed. The house-year is consumed year by year, but it is reproduced year by year without material fresh expenditure on the part of the landlord. This permanency alters the incidence of taxation.

If the demand falls off the landlord cannot remove his house—he cannot cease to produce his house-year, which therefore he must dispose of. Hence, in a stationary or declining community, where no new houses are being built, but where year after year a sensible proportion remains unoccupied, the landlord must sell his house-year unreservedly, and any tax imposed on house rent would fall on him alone; that is to say, he would receive a rent diminished by the full amount of the tax, and the tenant would pay no more rent for a house of a given class than if no tax were imposed. The supply curve becomes a straight horizontal line, and is unaffected by the tax; the demand curve is equally unaffected by

the tax; the number of houses let is unaltered by the tax, but the landlords lose as rent the whole amount raised by taxation.

This reasoning is based on the assumption, that the supply curve has become a straight horizontal line unaffected by the tax. This condition is altered in any prosperous or growing community. There, new houses must be built, and a considerable number of houses are always unlet, not because they are not required by the community, but because the speculative builders are holding out for higher terms. This produces a supply curve of the kind common to all other kinds of goods. At higher prices more goods are forthcoming. A newly imposed tax will then be distributed between sellers and buyers, landlords and tenants in a manner depending on the form of these curves. A sensible check will be given to the letting of houses, tenants will be content with somewhat less good houses, and landlords with rather smaller rents. This is the immediate effect of the tax—the greater portion would probably fall on the landlords at first, at least in the new houses where fresh contracts are being made. But after a few years the conditions would have altered. New houses are only built because the builders obtain the usual trade profit and interest on their capital—the check to letting consequent on the imposition of the tax will therefore diminish the supply of new houses until, owing to diminution in supply, rents have risen to their old average. Then builders resume their operations. The whole tax by that time will be borne by the tenants; that is to say, if there were no tax they would get their houses cheaper by the precise amount of the tax, because rents so diminished would suffice to induce speculative builders to supply them. The rents through the whole town are ruled by those of the new districts. There is a certain relative value between every house in the town, and if the rents of new houses are dearer the rents of the old houses are increased in due proportion. In fact, when new houses need to be supplied year by year, houses are commodities which are being produced, and the tax falls on the consumers.

The above principles determine the incidence of a tax, whether nominally levied on the landlord or tenant, but in their application account must be taken of the mental inertia of both landlords and tenants, as well as of the fact that many contracts for houses are

not immediately terminable. These two conditions will for the first few years after the imposition of any new tax cause it to fall on the party from whom it is nominally levied.

Precisely as a tax on trade not only falls on the traders, but injures capitalists and labourers, a tax on house rents injures the capitalists who build houses and the labourers they employ—not that the capitalist pays the tax, but he is prevented from finding a useful investment for his money owing to the diminution in the number or quality of houses required.

Taxes on Land.

The question of the incidence of taxes on land is peculiarly interesting. Land differs from all other commodities, inasmuch as the quantity of it does not depend on the will of any producer. The number of houses in a flourishing community does depend on the will of speculative builders; but land can only be increased in quantity by such processes as enclosing commons, or breaking up private pleasure grounds. We will neglect these small disturbing influences, and assume that all the land in a country is available for cultivation, where such cultivation is profitable; and that the absence of profit is the only reason for neglecting to cultivate any portion of it.

It is well known that the rent of each acre of land is simply the excess of annual value of that acre over the annual value of the poorest land which tenants think it worth while to cultivate. We may classify all land according to the total return which it will yield per acre upon capital invested in its cultivation; and we may draw a supply curve of land such that the ordinates will be the total quantities of land which will return each successive percentage on the capital required to cultivate it. The supply diminishes as the rate of percentage increases, that is to say, there is less land which will return ten per cent. on the capital than will return five per cent., and still less land which will return twenty or thirty per cent.

If, therefore, tenants as a body, considered as capitalists, will not cultivate any land which does not yield twenty per cent., there will be far less land in the market than if they will be just satisfied with ten per cent.

Again, all tenants are not of one mind, and we may construct a demand curve in which the ordinates are the total quantities of

land which would be let, if the land paying no rent be fixed at each successive percentage. The actual quantity of land let will be determined by the intersection of the two curves, and is represented by the height MD, fig. 4.

If we now build a solid on the base OD′DN, such that its height all along each ordinate x is the number of hundreds of pounds of capital per acre required to give the percentage corresponding to the length x, then we shall have a volume standing on (OD′DN), the contents of which will measure the total annual returns from all the land cultivated.* The rent is the volume standing on MDN, the profit received by the farmers is the volume standing on OD′DM, and this is in excess of what would have just tempted them to cultivate by the volume MDP. We may, therefore, considering the farmer as a capitalist and a trader, call the volume on MDP his trade profit, and the volume on OD′D the interest on his capital.

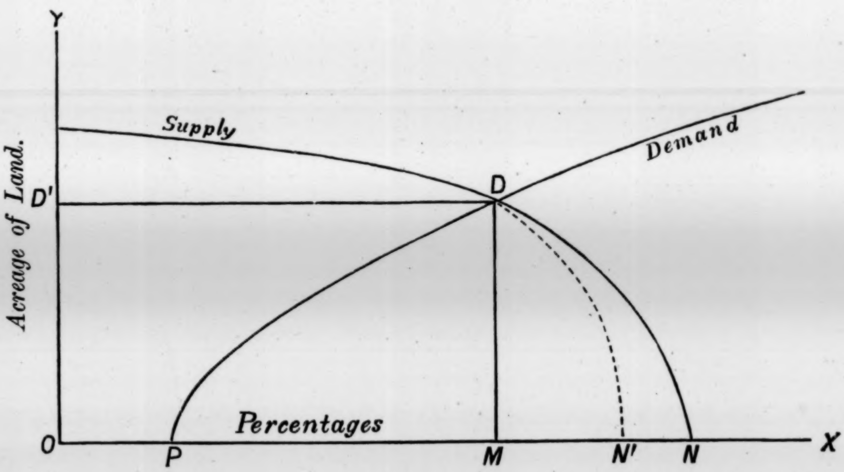


Fig. 4.

The effect of any tax on the land is to reduce the interest which each class of land is capable of returning on the capital employed. This it will do in very different ways according to the manner in which the tax is levied.

* If L.150 per acre are required to give the percentage x of any one class of goods, the height of the ordinate perpendicular to the plane of OD′DN will be 1·5.

If the tax be an *ad valorem* duty on rent, it will modify the supply curve only between D and N. There will remain just as much land as before capable of paying rates of interest less than OM, but the quantity of land capable of paying the higher rates will be diminished; in other words, the rate of interest which the poorest land worth cultivating pays will not be affected, for this land pays no rent and remains untaxed—hence no land will be thrown out of cultivation, but the supply curve will be altered from DN to DN′, diminishing the volume representing rent, but leaving the other quantities untouched; hence any tax assessed on rent is paid wholly by the landlord. The amount of the tax is the volume standing on DNN′. It is curious to remark that this tax in no way falls on the consumer. The tax on rent simply diminishes the excess of value which some land has over others; no land is thrown out of cultivation, and no less capital employed in production than before; no one suffers but the landlord. If, instead of being assessed on the rent, the tax is assessed on the produce of the cultivation, the incidence of the tax will be greatly modified. The cultivation of land will no longer be so profitable; *i.e.*, the returns from capital employed on the land will be less; in other words, the whole supply curve of the land will be modified, falling everywhere if the produce taxed be that which is produced on all qualities of land. Some land will fall out of cultivation, and only part of the tax will be borne by the landlord; part will fall in the first instance on the tenant, but he, like any other manufacturer, will recover almost the whole of his portion from the consumer. Tenants will be injured by the limitation of the number of transactions, and labourers by the diminution in the amount of work required. This is the effect of an octroi duty.

Sometimes a tax is assessed not on the rent, but on an assumed value per acre. Such a tax can never be raised on land which pays no rent, for the owner would rather abandon possession of the land than pay the tax. It might, however, lead to the abandonment of the cultivation of poorer soils; it would then injure tenants and consumers, although they would not pay one penny of the tax; for taxes cannot be paid out of lands which lie waste; assuming that the tax is always less than the rent, as it certainly always should

be, it will be paid wholly by the landlords. The tax in this case does not diminish the supply of land.

A cognate question of great interest is, Who reaps the benefit of any improvements in agriculture, making land return more than it previously did? This improvement may require, and probably will require, increased investment of capital. The whole supply curve will be raised; assuming the demand to remain the same, fig. 5, M″D″ will be the new increased number of acres in cultivation, but land will be left uncultivated which would have returned the interest OM on capital. The volume standing on D′D″N″ will be much greater than that on D′DN, for the third dimension will also have increased; the average rate of interest and the trade profit of the tenant will have increased, and it is highly probable that the volume standing on D″M″N″ may be greater than that which stood on DNM; but this is by no means certain. It might at first be actually smaller. In all probability, however, the demand curve is very nearly vertical, a small increase of profit tempting a

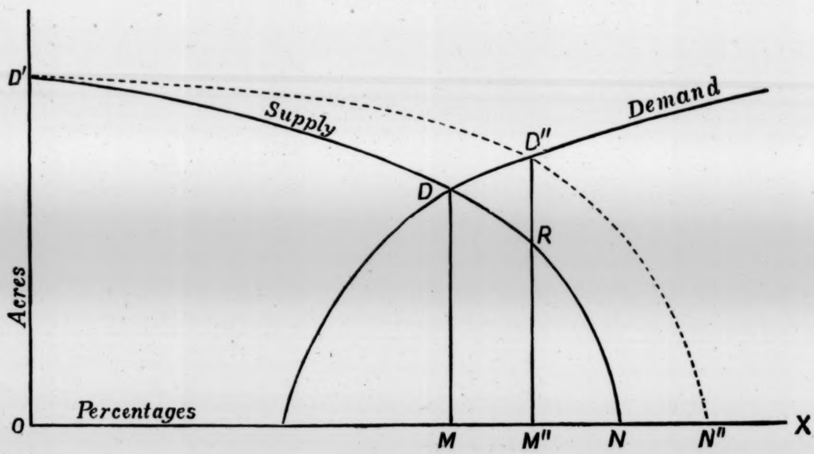


Fig. 5.

largely increased investment of capital in farming. If this be so, then the landlord also reaps considerable benefit from the improvement, for if the farmers were contented with nearly the same rate of interest as before, the solid standing on DRNN″D″ which he gains would be larger than the solid on DRM″M which he loses; moreover, the volume on RNM″, which he retains, is increased. Labourers and consumers also gain.

4. Additional Notes on the Occurrence of the Sperm-Whale in the Scottish Seas. By Professor Turner.

In a communication made to this Society on the 6th February, 1871, I noted the capture of a sperm-whale at Oban in May, 1829, and I collected from various sources records of the stranding of seven additional specimens on the Scottish coasts.

Since that communication was published, a large sperm-whale has come ashore on the west coast of the Isle of Skye, some particulars concerning which I propose to relate in this communication.

Tourists in Skye, during the past autumn, who visited Loch Corruisk by boat from Torrin, as they sailed up Loch Scavaig, became conscious, by another sense than that of sight, that a large animal in a state of putrefaction was in their immediate vicinity.

A correspondent of the "Glasgow Herald," writing in July last, states that a great whale entered Loch Scavaig about the middle of that month, and after floundering about, bellowing like a bull amongst the rocks, amidst which it had become entangled, it died after a lapse of two or three days. Large quantities of blubber were removed from the carcase without loss of time by the neighbouring fishermen, but enough of the external form remained to enable the correspondent to give the following description: Skin black, thick and corrugated. Head enormous, square, ending in a flat snout some eight or ten feet across, looking like a peat stack. Eye small, surrounded with lashes, some 16 feet from the snout. Blower covered with a flap a foot long. Under jaw slender, shorter than the upper, in it were thirty-six teeth shaped like the ends of ducks' eggs. No teeth were visible in the upper jaw. The whale could not be short of 60 feet in length.

My attention having been directed by Sir Robert Christison to the newspaper report, I at once recognised from the form of the head, jaw, and teeth, that the characters were those of the sperm-whale (*Physeter macrocephalus*), and I determined, if possible, to obtain a portion, if not the whole of its skeleton. The distance, however, of the spot, where the carcase was lying, from human habitations, and the want of proper appliances for lifting heavy objects, have proved hindrances to the removal of the huge cranium of the animal, but the two halves of the lower jaw, and a number

of the smaller bones of the skeleton, are now in my possession.

From the examination of these bones an estimate may be formed of the age, size, and, I believe, also the sex of the animal.

The state of ossification of the bones proved that the animal had reached its full period of growth, for the epiphysial plates were anchylosed to the bodies of the vertebræ, the lower jaw had attained a great length, the radius and ulna were anchylosed together, both at their upper and lower ends, and the various subdivisions of the sternum were welded into one massive bone.

As some estimate may be formed of the size of the animal from the dimensions of its lower jaw, it may be useful to give the measurements of this bone, and at the same time to compare it with the dimensions of some other specimens.

In the Natural History department of the Edinburgh Museum of Science and Art is a magnificent lower jaw, which was presented many years ago by Captain William Hardie. It possesses twenty-five teeth on one side, but only twenty-four on the other. On the outer face of the right mandible there has been engraved a large picture of the boats of the ship "Woodlark" of London, Captain William Hardie, engaged in the capture of the sperm-whale, in a school of sperm-whales, off the Banda Islands, April 7th, 1843. On the other half, a figure, 43 inches long, of a sperm-whale has been engraved. As authentic drawings of this animal are by no means common, and as this figure has been executed with a considerable amount of artistic skill, and in all probability by one well acquainted with the form and proportions of this animal, I produce on the following page a reduced copy. In the Anatomical Museum of the University of Edinburgh is the mandible of a young male, presented two years ago by my pupil, Mr F. B. Archer of Barbadoes. The animal was captured in the North Atlantic Ocean, in the latitude of the Azores.

Professor Flower has also carefully recorded * the dimensions of three specimens from Tasmania, in the Museum of the London College of Surgeons, one of which is stated to be "unique on account of its great size," and in measuring the Edinburgh specimens I have followed his plan of taking the length from the apex

* Trans. Zool. Soc. 1868.

Fig. 1.

Reduced copy of the figure of a Sperm Whale, engraved on the mandible in the Museum of Science and Art, Edinburgh.

of the mandible to the middle of a line drawn across the posterior ends of the rami.

	Entire Length.	Length of Symphysis.	Greatest Girth Behind.
Mandible from Isle of Skye,	190½″	116″	56″
Proportion, ...	100	61	29
Mandible in Natural History Museum,	196	120	54
Proportion, ...	100	60	27
Mandible in Anatomical Museum,	80	38½	29
Proportion, ...	100	48	36
			Width Behind.
Mandible, young skull, Tasmania,	49	21	31
Proportion, ...	100	43	63
Mandible, Tasmanian Skeleton,	174	102	72
Proportion, ...	100	59	41
Largest Tasmanian Mandible,	194	124	75
Proportion, ...	100	64	38

The specimens in the Edinburgh Museums corroborate the conclusions arrived at by Mr Flower, that a gradual increase in the length of the symphysis, compared with that of the entire jaw, takes place as age advances, and it is obvious also that the girth behind diminishes in proportion to the increase in the length of the jaw. This increase is without doubt co-ordinated with the development and growth of the teeth.

Although the teeth had been removed by the fishermen, and sold to tourists before the mandible of the Skye sperm-whale came into my possession, yet the sockets were entire, and twenty-four on each side could be counted, so that the animal had attained its complete dentition. Seven loose teeth were, however, sent, all of which, with one exception, were worn on the surface and sides of the crown. In all, the pulp cavity was completely closed at the extremity of the fang, and, in several, irregular outgrowths from the surface of the fang were present. Two of the teeth, though worn at the crown, closely corresponded in general form with the one not so affected, and were much more slender and tapering than the remaining four, the roots of which were much more bulky. The unworn tooth was five inches long, and the greatest circumference of its root 4¼ inches.

The sternum was a massive, plate-like, triangular-shaped bone, greatly expanded anteriorly in its transverse diameter, and gradually tapering backwards to a rounded apex posteriorly. Inferior surface, convex; superior, concave; anterior border, convex; lateral borders varied in thickness, but were from four to five inches in diameter at the thickest part. Four well-marked costal articular surfaces on each lateral border. An oval hole, 6½ inches long, was in the middle of the manubrial element of the bone, and 4½ inches

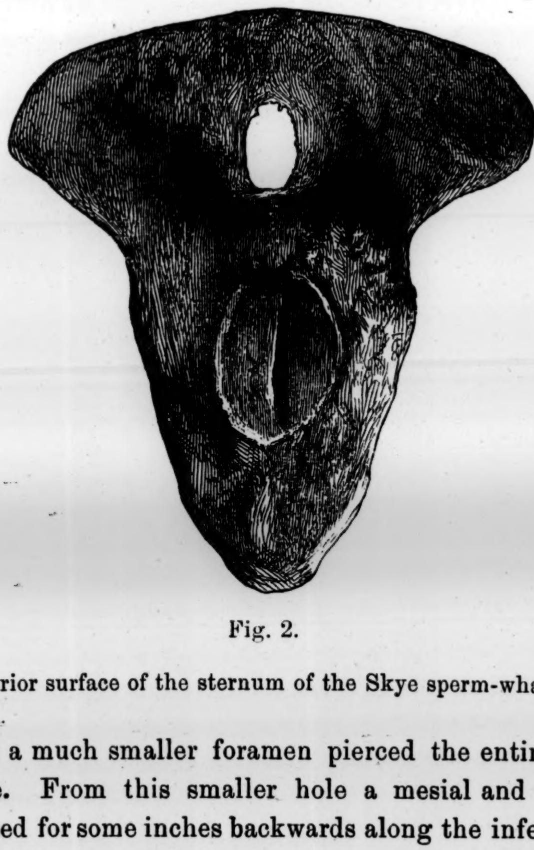


Fig. 2.

Inferior surface of the sternum of the Skye sperm-whale.

further back a much smaller foramen pierced the entire thickness of the bone. From this smaller hole a mesial and two lateral grooves passed for some inches backwards along the inferior surface of the bone. On the inferior surface there was no indication of the original transverse segmentation; on the superior surface, 19 inches in front of the posterior end, a deep transverse fissure passed across the bone through the middle of the third pair of costal articular facets, but there was no trace of the original division between the first and second segments.

Extreme length of sternum, 50 inches; transverse diameter at

first pair of costal facets, 40 inches; at second pair, 22 inches; at third pair, 18 inches; at fourth pair, 14 inches. This bone had attained a more complete stage of ossification than had previously been described or figured in the sternum of this cetacean.

The length of the third transverse segment of the sternum being 19 inches, I examined it carefully to see if any evidence of a subdivision into smaller segments could be detected, but without success. Moreover, I find that Professor Flower has met with

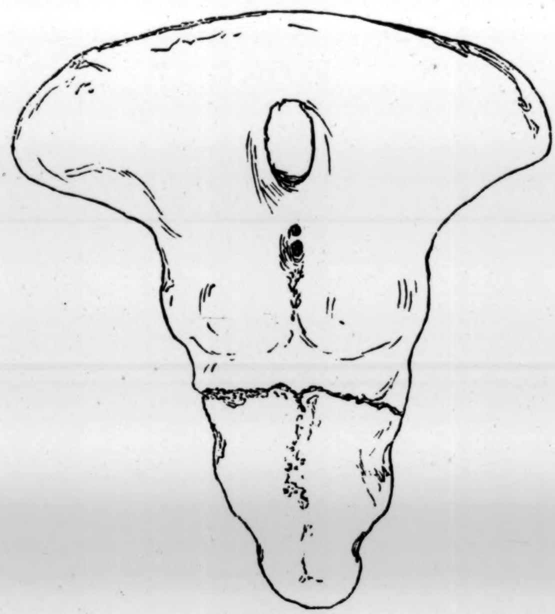


Fig. 3.

Outline sketch of the superior surface of the sternum of the Skye sperm-whale.

great differences in the length of the terminal segment of this bone in the specimens which he has examined. In one from Tasmania the length was 14¾ inches, whilst in the Caithness Cachalot the hinder piece is represented by a median spheroidal nodule of bone, 4 inches in diameter, imbedded in dried cartilage. The terminal piece of the sternum is therefore variable in its dimensions, and the greater length in the Skye specimen is without doubt due to the age of the animal having rendered possible complete ossification of the terminal cartilage.

That the animal had reached its full growth and attained the

adult period of life is evident from the completed ossification and the dimensions of its bones. There can be, I think, little doubt but that it was of the male sex. For although little has been done in the descriptions of the sperm-whale to discriminate the sexual characters of the skeleton, yet those who have had opportunities of observing the habits of this cetacean, agree in ascribing to the male a much greater magnitude than is acquired by the female. That excellent naturalist, Mr F. D. Bennett, for example,* states that the adult female does not exceed the length of thirty, or at most thirty-five feet.

We may now pass from the most recent specimen to the consideration of, I believe, the most ancient relic of the sperm-whale which has yet been found in the British Islands.

In August 1871, Mr George Petrie of Kirkwall presented to the Royal Scottish Society of Antiquaries a tooth recently obtained from a "brough" near the Howe of Hoxa, in the Isle of Sh. Ronaldsay, on a promontory opposite the Bay of Scapa. This tooth had obviously been buried in the earth for a lengthened period, and in all probability was co-eval with the early occupation of the "brough," and may have belonged to one of its early Norse, or even still more ancient inhabitants. This tooth has been carefully examined by Professor Duns, Dr John Alexander Smith, and myself, and we all agree in regarding it as the tooth of a sperm-whale. A part of the alveolar end of the tooth, more especially on one side, has been broken away, so that the conical-shaped pulp-cavity is fully exposed. The free end of the crown is smooth and rounded, such as one sees in specimens of well-worn teeth of this animal. The length of the tooth is $5\frac{3}{4}$ inches, but, owing to a part being broken off, this does not give its full length; the greatest girth is $6\frac{3}{4}$ inches.

Mr Petrie has most courteously sent me an account of the locality in which he discovered the tooth. He says:—"I was glad to find that the tooth was of some interest. I was led to its discovery by a request of my friend, Mr James Fergusson, the author of the 'Handbook of Architecture,' to make some excavations in the vicinity of the Howe of Hoxa, with the view of discovering, if

* Whaling Voyage, vol. ii. p. 155.

possible, the tomb of the celebrated Orkneyan Jarl, Thorfinnr who was, according to the 'Orkneyinga Saga,' buried at Haug seið, now known as the Howe of Hoxa. The Howe is apparently a long-shaped natural mound of considerable height, on which artificial mounds were probably made, as traces of them can still be seen, as well as of a massive stone wall encircling a great portion of the top of the mound. On the north end of the mound are the ruins of a large circular structure, which, on being excavated between twenty and thirty years ago, was found to be the remains of a brough or round tower. On proceeding to the spot last summer, and carefully examining the mound, I found that it would involve much time, labour, and expense to make a satisfactory examination. I determined, therefore, to excavate a smaller mound, evidently wholly artificial, at a short distance from the Howe of Hoxa, but connected at one time with it, as traces of an avenue of stones leading from the one to the other were still to be seen. I expected to find a chambered tomb, but to my surprise a structure resembling the ordinary brough, but far less symmetrical than such buildings usually are, was revealed. I am inclined to think that it was sepulchral in character, although of a type unique, so far as my experience goes. The passages or galleries were still roofed in many parts by flagstones laid across from wall to wall. The excavations did not produce many relics, but amongst these were bits of dark pottery and several vertebræ of whale much scorched by fire. One of the vertebræ, about 1 foot in diameter at the broadest part, and 9½ inches in height, had been fashioned into a rude vessel by scooping out the central or more porous part of the bone, as is often the case. It was found about two feet beneath the surface of the mound at A, on what appeared to be the floor of the interior of the structure, and it and the other vertebræ were buried beneath the ruins, which seemed to have fallen upon them. The tooth was found at B, and not far off a piece of freestone, convex on one side and slightly concave on the other. The concave side was tolerably smooth, apparently due to friction of a freestone rubber passing frequently over its surface. Similar stones were found in the brough of Hoxa, when it was cleared out some years ago. They much resemble the slightly hollowed stones found at New Grange, in

Ireland. I do not remember any case of a brough which has been explored in Orkney in which bones of the whale have not been found."

"I hesitate very much to attempt even to assign a date to the

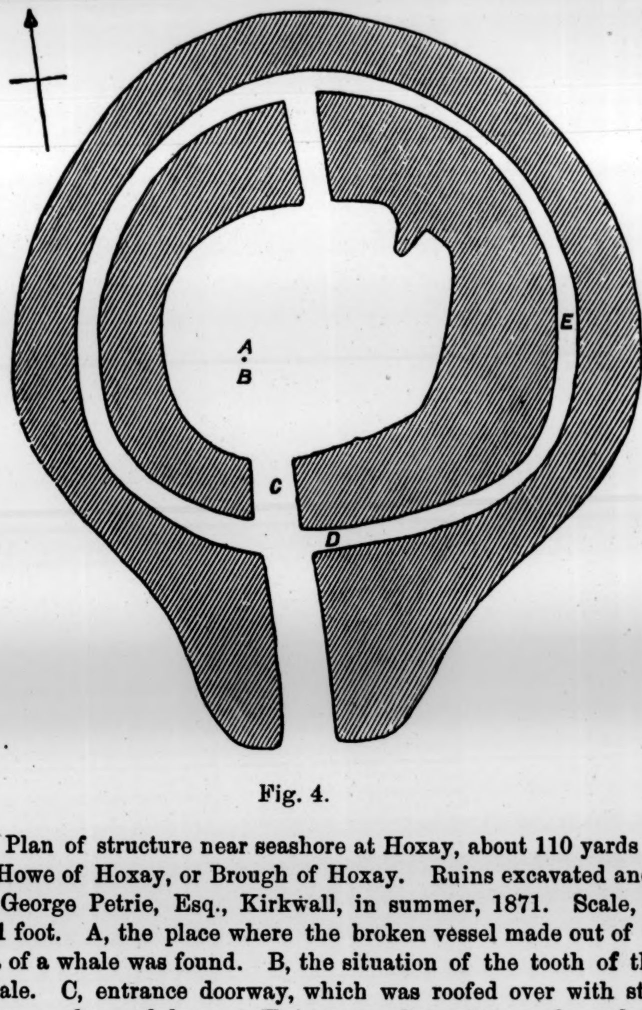


Fig. 4.

Ground Plan of structure near seashore at Hoxay, about 110 yards westward of Howe of Hoxay, or Brough of Hoxay. Ruins excavated and planned by George Petrie, Esq., Kirkwall, in summer, 1871. Scale, $\frac{1}{18}$th inch to 1 foot. A, the place where the broken vessel made out of the vertebra of a whale was found. B, the situation of the tooth of the sperm-whale. C, entrance doorway, which was roofed over with stones. D, passage, also roofed over. E, passage where stone roof was destroyed.

structure in which the tooth was found. It may belong to the period when the Celtic or Pictish population by whom the islands were occupied prior to their invasion by the Scandinavians, but I do not think, from the general appearance of the ruins and the character of the remains found in them, that the tooth belonged

to a whale captured or driven ashore later than the Scandinavian-Pagan period in Orkney, or say the ninth or tenth century."

As bearing on the early history of the sperm-whale in the British islands, I may next refer to a passage in a memoir by the eminent Norwegian archæologist, Professor P. A. Munch, to which my attention has been directed by Mr Joseph Anderson, the curator of the Antiquarian Museum. The memoir is entitled "Geographical Elucidations of the Scottish and Irish Local Names occurring in the Sagas,"* and on pp. 128, 129, Professor Munch, in his account of the Shetland Isles, says:—"The island of Yell is nearly divided into two halves by the deep fiords which penetrate on each side, Whalefirth (Hvalfjörðr) on the west, and Reafirth (Reyðarfjörðr) on the east. In a deed dated May 19, 1307, which speaks of the pledging of the estate Kollavâgr, now Cullavoe, one of the witnesses is a Högni i Reyðarfirði. This Reyðarfjörðr is clearly the above Reafirth, early contracted, or rather corrupted, even by Norse speakers, to Rafjörd." Further, Professor Munch states, it is very suitable that the two opposite fiords should be called, the one Hvalfjörðr and the other Reyðarfjörðr, for Reyðr (now called Röðr or Rör, in Norway), is also a kind of whale, the *Physeter macrocephalus*, black-headed spermaceti whale.

If we are to accept this interpretation by Professor Munch, that the old Norse term Reyðar was equivalent to our sperm-whale, then we should have to assume that this cetacean was so well known to the ancient Norsemen that they had coined a word to designate it. And it is indeed not improbable that, considering their roving habits, they may have sailed in the seas which it most usually frequents, and perhaps have chased it for the sake of its valuable oil.

But from the association of this name with a particular firth in the Shetland group of islands, it would, granting the accuracy of Munch's interpretation, seem as if, in the early years of the Norse settlement, the sperm-whale had not unfrequently entered this firth, and perhaps been captured there—a circumstance which would show that this animal was then a much more frequent visitor of

* Mémoires de la Soc. Royale des Antiquaries du Nord, 1850–1860, Copenhague.

the Scottish seas than we know it to be at the present day, or indeed to have been for some centuries past.

But I think it very questionable if the interpretation given by Professor Munch of the old word Reyðar can be regarded as zoologically correct. Torfæus, the historian of Greenland, in his account of the cetacea which frequent the Greenland and Iceland seas,* uses the term Reidr three times in his description of these whales. One he terms Hrafnreidr, white in colour, of a length of fourteen or sixteen cubits, "branchiis etiam præditus," and tastes well. A second, called Hafreidr, a whale of sixty cubits, or a little more, which carries a small horn, and is most pleasant to eat. The third is named Reidr, or most usually Steipireidr, which, he says, surpasses all others in sweetness, is gentle, and not to be feared by ships. The largest which has been caught by the Northmen equals 130 cubits, is very fat, "branchiis gaudet," but wants teeth. This description by Torfæus is much wanting in precision, and the statement that the Hrafnreidr and Reidr possess branchiæ would lead one to say, if this term were understood by him in the sense in which it is now employed, that these animals were not whales, but fishes. It is probable, however, that the so-called branchiæ in Hrafnreidr and Steipireidr may be the plates of whalebone which depend from the roof of the mouth of the baleen whales, and which have a laminar arrangement not unlike the gills of a fish, and might readily be mistaken for such by an inexperienced observer. The absence of teeth, however, conclusively shows that these could not be sperm whales.

Otho Fabricius, in his "Fauna Groenlandica,"† identifies the Hrafnreidr of Torfæus with the fin-whale named by Linnæus *Balæna boops;* and the Reidr or Steipereidur with the *Balæna musculus* of the same naturalist. By Otho F. Müller,‡ the term Reider or Reydur is applied to two species of Baleen whales. Mohr also, in his Natural History of Iceland,§ adopts the classification of Fabricius; and Erik Jonsson, in his Dictionary of old Norse terms,|| accepts the definition of the above naturalists. Further,

* Gronlandia Antiqua, pp. 90, 96. Havniæ, 1706.

† Hafniæ, 1780, p. 36, *et seq.*

‡ Zoologicæ Danicæ prodromus. Hafniæ, 1776.

§ Forsog til en Islandsk Naturhistorie. Copenhagen, 1786.

|| Oldnordisk Ordbog. Copenhagen, 1863.

both the lexicographer and the naturalists agree in giving as the Norse equivalent for our term sperm-whale, not Reyðar, but Búrhvalr. Munch himself, also, by putting the Norwegian term Röhr or Rör as equivalent to the older word Reyðar, supplies me with an additional argument against the latter word being regarded as signifying sperm-whale, for Rör or Rörhval is merely our term Rorqual, *i.e.*, a whale with folds and sulci extending longitudinally along the belly, such as one sees in the Balænopteridæ or Finner whales, but which do not exist in the sperm-whale.

Hence we cannot regard Reafirth in Yell as having received its name from having once been a place of resort for the sperm-whale, or as affording any evidence that our seas were at one time more largely frequented by these huge cetaceans than at the present day.

But though this name loses its interest in connection with the natural history of the sperm-whale, it acquires importance in reference to the natural history of the rorquals. Of this group of whales, two, viz., the common Finner, and the species of Fin whale, of which we had recently so fine a specimen stranded at Longniddry, attain a length of upwards of 60 feet, and are not uncommon in our seas. By modern zoologists, the common Finner is usually called *Balænoptera musculus* (*Physalus antiquorum*), and may be identical with the Hrafnreidr of Torfæus. The other, the *Balænoptera Sibbaldi*, has been identified by Professor Reinhardt and myself * as identical with the Rorqual, to which the Icelanders even at the present day apply the name of Steypireythr. In all probability the firth on the east side of Yell, now known as Reafirth, was frequented by these Rorquals, and was named by the ancient Norse settlers, Reyðarfjörðr, from this circumstance, whilst the deep inlet of the sea on the west side of the island, now known as Whale-firth, may have obtained its Norse name of Hvalfjördr from having been the resort of the "caaing" whale, which in large herds still frequents the Orkney and Shetland seas, and is killed in great numbers by the islanders.

For convenience of reference, I may append a tabular statement, compiled from the cases referred to in this and my former essay,

* See my Memoir in Trans. of this Society, p. 247, 1870.

of the well-authenticated instances in which the sperm-whale has been met with on the Scottish coasts.

Locality.	Date.	Authority.
Hoxay, Orkney, ...	9th or 10th cent.?	George Petrie.
Limekilns, ...	Feb. 1689	Sir R. Sibbald.
Cramond, ...	1701	James Paterson.
Monifieth,*	Feb. 1703	Sir R. Sibbald.
Ross-shire, ...	1756	Sir W. Jardine.
Cramond, ...	1769	James Robertson.
Hoy Sound, Orkney, . . .	About 1800	George Low.
Oban, ...	May, 1829	William Turner.
Thurso, ...	July, 1863	J. E. Gray, and W. H. Flower.
Loch Scavaig, Skye, . .	July, 1871	William Turner.

Monday, 5th February 1872.

SIR WILLIAM THOMSON, Vice-President, in the Chair.

At the request of the Council Professor Tait gave an Address on Thermo-Electricity.

The following Communication was read:—

1. Note on Cystine. By James Dewar, F.R.S.E.

The following observations on Cystine are a continuation of those formerly communicated to the Society by Dr Arthur Gamgee and myself, during the course of the Session 1869–70, and reprinted with addition in the "Journal of Anatomy and Physiology" for that year; and although really little of a novel nature to present to the Society, still it is necessary to show some additional facts have

* In connection with this animal, I may refer to an essay in the "Scottish Naturalist," dated November 1871, by Mr Robert Walker, of St Andrews, in which he describes and figures the vertebra of a whale, in the University Library of that city, which he believes to be the tenth dorsal of a youngish Cachalot. He believes it to be a relic of a whale stranded there, from which Mr Foster, a former Regent in the University of St Andrews, obtained a parasite which he sent to Sibbald, who figured it. He thinks that the whale figured on the same plate, though stated to be stranded at Monifieth, may have been this animal.

been observed tending towards the synthesis of this interesting substance.

The most important fact ascertained with regard to the chemical relation of cystine in memoir referred to was the production of pyruvic acid, when it was treated with nitrous acid. In this reaction the amido residue was not alone eliminated, the sulphur also separating as sulphuric acid, however carefully the experiment was performed. The fear of allowing the action to proceed too far, on the necessarily small quantity of substance operated upon, prevented us from purifying the product thoroughly, and, consequently, the analysis differed slightly from that of pure pyruvic acid. We had no hesitation in saying, however, the acid agreed better with the chemical characters of the syrupy modification of pyruvic acid than with that of Wischelhaus's carbacet oxylic acid, that we had anticipated would be produced, and that in all probability cystine would be found to be an amido-sulpho pyruvic.

If cystine is directly related to pyruvic acid, it must contain five instead of seven hydrogen atoms (and this supposition agrees well with the published analysis). The formula of the compound will then be, $C_3H_5NO_2S$. On this supposition, we may derive from pyruvic acid at least five isomers, that will all have the general characters of cystine, although there are many other possible constitutional formulæ.

Pyruvic Acid.	1.	2.
CH_3	CH_2NH_2	CH_2NH_2
CO	CO	CO
$CO.OH$	$CO.SH$	$CSOH$
3.	**4.**	**5.**
$CH_2\,(NH_2)$	$CH\genfrac{}{}{0pt}{}{NH_2}{SH}$	CHS
CS	CO	$C\genfrac{}{}{0pt}{}{NH_2}{H}$
$CO.OH$	$CO.OH$	$CO.OH$

Of the five possible cystines formulated, it is impossible to select that of the natural substance, because of our ignorance of the intermediate sulpho-acid. All attempts to replace the amido group alone by the action of nitrous acid having failed, I have tried several experiments, with the object of replacing the sulphur alone, with the small quantity of cystine at my disposal.

If cystine is one of the above five substances, the replacement

of the sulphur by hydrogen will generate very different bodies. Theory enables us to predict that, in the case of bodies having the constitutional formulæ of No. (5), we ought to obtain alanine. In that of (3) (β) alanine, and in that of (4) amido-lactic acid (serine), and in that of (2) amido-glycerine; whereas it is difficult to imagine the sulphur in (1) being replaced. A successful experiment in this direction ought to restrict the selection to two possible constitutional formulæ in the worst case, and synthetical processes might then be attempted. It was formerly observed that nascent hydrogen generated in an acid solution, readily liberated sulphuretted hydrogen, and might be used as a test for this substance. The action goes on, however, very slowly, and it was found extremely difficult to get anything like the theoretical quantity of sulphur evolved. With this experience, sodium amalgam suggested itself as being more powerful, and equally likely to act. When cystine is dissolved in caustic soda, and sodium amalgam added, in a few minutes it is easy to detect the presence of a sulphide by the nitro-prusside test. The action was allowed to proceed for several days, being occasionally rendered acid by the addition of hydrochloric acid, and the amalgam renewed. Ultimately the alkaline solution, after being neutralised with hydrochloric acid, was evaporated and treated with boiling alcohol to separate the chloride of sodium, and to dissolve any hydrochlorate of alanine that might be formed. After the filtrate was evaporated, the residue still contained sulphur, from the presence of hydrochlorate of cystine. This was separated by treating with water, and the filtrate was boiled with oxide of lead, treated afterwards with sulphuretted hydrogen to precipitate the dissolved lead, and evaporated. The residue was then heated to 200 C. in a tube, with the object of subliming the alanine. No crystalline sublimate was observed; it is probable, therefore, that substances of the constitutional formulæ of 5 do not express the constitution of normal cystine. This result is subject to a certain amount of reservation, from the difficulty of separating a small quantity of substance from a very large amount of secondary material accumulated in the course of the experiment. The battery is far better adapted to give a supply of nascent hydrogen in this case; and an experiment made in this way looks promising, if sufficient material was to be had.

The small quantity of substance left I have employed for the purpose of corroborating the production of pyruvic acid, when it is treated with hydrate of baryta.

Took a decigramme of cystine, treated it in a tube with a solution of hydrate of baryta, and heated it all night to a temperature of 130° C., opened it, and transferred contents to a beaker, boiled to expel the ammonia produced, then added an exactly equivalent quantity of sulphuric acid, filtered from the sulphate of baryta; after boiling to expel the sulphuretted hydrogen, the filtrate evaporated contained a yellow syrupy acid, which contained a few crystals under the microscope, having the appearance of Finck's uvitic acid. Ammonia was added, and gave a yellow solution, which was evaporated on the water-bath; it was dissolved in water, and gave a white precipitate, with nitrate of silver, which was not distinctly crystalline; it also gave a white precipitate with subnitrate of mercury, and a red colour with a crystal of sulphate of iron, and no precipitate with sulphate of copper. The barium salt was also found to be non-crystalline, the acid lost the power of giving a red colour with Ferric salts after treatment with sodium amalgam, and the composition of the silver salt agreed better with pyruvic acid then formerly.

Considerable progress has been made in an examination of the chemical characters and relations of the thio-pyruvic acids. Normal thio-pyruvic acid has been obtained from the di-chlorpropionic ether. When this ether is treated with excess of alcoholic sulphide of potassium, we obtain at once a precipitate of chloride of potassium, and a solution of the potash salt of the new acid. When this is diluted with water, acidulated with sulphuric acid, and shaken up with ether, the acid is obtained in yellow crystalline plates, part of it seems to remain a viscid fluid. The lead and silver salts are white and insoluble, blacken when heated. It precipitates mercurous salts black from the first. The calcium, barium, iron, cadmium, and copper salts are all soluble. The potassium and sodium salts are intensely yellow, and decompose slightly on exposure to the air. When treated with tin and sulphuric acid, they evolve sulphuretted hydrogen.

The thio-carboxyl pyruvic acid has not yet been obtained in a pure state. When pyruvic acid treated with pentasulphide of phosphorus, a violent action takes place, associated with much

frothing; and when the product is distilled, a large mass of carbon is left in the retort, and a very small quantity of distillate is obtained. It is probable that chloro-pyruvil, when treated with sulphide of potassium, will give a more satisfactory yield. It is the author's intention to make a careful comparison of these two acids, and to transform them into amido-acids, with the object of making an artificial cystine; and the results arrived at will shortly be communicated to the Society.

The author's stock of cystine being now exhausted, he will feel extremely indebted to any one who would spare him a small quantity for experimental purposes.

The following Gentlemen were elected Fellows of the Society:—

George Forbes, Esq., B.A., St Catherine's College, Cambridge.
J. Lindsay Stewart, M.D., Conservator of Forests, Punjab.
Rev. Charles R. Teape, M.A.

Monday, 19th February 1872.

Principal Sir ALEXANDER GRANT, Bart., Vice-President, in the Chair.

The following Communications were read:—

1. Remarks on Contact-Electricity. By Sir William Thomson.

2. On the Curves of the Genital Passage as regulating the movements of the Fœtus under the influence of the Resultant of the Forces of Parturition. By Dr J. Matthews Duncan.

The observer of the current literature of Midwifery finds nothing more characteristic of it than the number of papers on the mechanism of natural parturition. These papers indicate for the most part an enlightened zeal, for they are engaged with a most important branch of this mechanism, namely, the mode of action of the force of labour upon the fœtus and upon the passages, and the explanation thereby obtained of the changes which take place in these as natural labour advances.

For these inquiries great additional value would accrue, were the amount of power exerted by the combined forces of parturition

well known; but they can be carried on to a great degree of advancement, even while the amount of power exerted by the machine is unknown, or at least unsettled.

Some of these inquiries as to the action of the force of labour upon the fœtus and passage are very easily solved, and have been long in this condition. But the most, and by far the most, important are questions only recently raised; and of which it may be said that few are familiar to the profession even as questions, and still fewer can be regarded as settled. These inquiries form the natural sequel to the most recent developments of our knowledge of natural parturition. These have been chiefly engaged in describing how the fœtus and the passages actually behave during the process, while the new inquiries are destined to explain why they so behave. These new inquiries will introduce us far more deeply into the subject of the mechanism of labour than those which have preceded them. They are specially difficult because of the varying conditions of the force of labour and of the correlated parts, the fœtus and the passage. The former has the relations of its parts extensively changed while the process of labour proceeds, and the latter is only produced at the time by what is called the development of parts, as the fœtus advances.

The subject to which I wish at present to direct attention is the curves of the genital passage, and their influence on the phenomena of parturition.

I. The first curve to which I direct attention is said to be at the brim of the pelvis, and to have its convexity directed downwards and forwards. I do not admit that the curve exists, but it is of the utmost importance to decide the point, because, without doing so, we cannot possibly determine the primary direction of the driving force of labour. Hitherto and now, the axis of the gravid uterus has been and is generally regarded as coincident with the axis of the brim of the pelvis, and to indicate the direction of the resultant of the forces of parturition. But an elaborate attempt has been recently made by Schatz and Schultze, especially by the former of these authors, to demonstrate that the axis of the uterus at rest and in action is inclined to the axis of the brim of the pelvis, at a small angle opening forwards and upwards, and of about ten degrees. I have just said that the axis of the uterus has been generally considered to indicate the primary direction of the driv-

ing power; but it is evident that this can only be the case if a variety of conditions be satisfied. Of these the following are probably principal:—the assistant driving force, which is auxiliary to the proper uterine force, must be also directed in the axis of the brim of the pelvis, being supposed to be uniformly applied to the uterus by the circumjacent viscera and parts, acting like a fluid, exerting pressure equally in all directions: the uterus must be distended with a fluid which is copious enough to prevent any part of the walls being specially pressed upon or indented by the fœtus; or, it must have its tendency to become spheroidal superiorly unrestrained. Now Schatz, in addition to giving the proper uterine driving force a posterior inclination to the axis of the brim by ascribing to the uterine axis such an inclination, still further increases the inclination of the whole driving force, by describing the special direction of the auxiliary bearing-down driving force as still more inclined than the direction of the uterine axis. The resultant of the combined or whole driving forces will of course, according to Schatz, have a direction somewhere intermediate between that of the uterine and that of the auxiliary driving forces.

Smellie's authority is much relied upon in support of the existence of this curve. In his plates he gives the uterus this inclination to the axis of the brim of the pelvis, both in natural cases and in cases of deformity; but this is not satisfactory evidence as to what he believed, for it is probable that in preparing his plates he did not pay particular attention to the point. Those of them to which reference is here made (as xii. and xiv.) are not in the proper sense drawings or pictures, but mere plans, and might very well have been arranged as they are, merely because in other respects the works looked well. Dr Barnes, in his recent work on obstetric operations, while adhering to the generally entertained view as to the coincidence of the axis of the uterus and of the brim of the pelvis, implies, by his descriptions and drawings, a belief that, in most if not all cases of antero-posterior contraction of the brim of the pelvis, the uterine axis is inclined to the axis of the contracted brim, as Schatz believes it to be in cases generally. This is not the place for any full criticism of what Barnes very aptly calls the curve of the false promontory, because I confine myself to ordinary or natural conditions. I shall merely say that this important and practically valuable doctrine of Barnes regarding the curve of the false promon-

tory is made too general. It can be true and applicable only where the posterior uterine obliquity is present, and it is not demonstrated, nor is it probable that this always is so, in cases of deformity.

It is extremely desirable that means should be devised for ascertaining the direction of the resultant of the combined forces of parturition, and especially of the axis of the uterus in action. The means adopted by Schatz with this object in view are not satisfactory; they merely go the length of showing how carefully he entered upon the question. But it may be permitted me to state reasons which tend to establish the ordinary opinion, and to discountenance that of Schatz.

If the uterine axis is inclined to the brim of the pelvis posteriorly to its axis, we should expect to find the child's head at the commencement of labour, while yet above the brim, to be in a position which has never, so far as I know, been ascribed to it in natural cases. Smellie, in his plate xii., gives this position consistently, but not truly. He could not avoid doing so, unless he represented the child at rest as having a left lateral flexion of the head, which would be ridiculous. His mode of drawing the uterus with this posterior obliquity created an exigency for him, which he could get over only by what must be regarded as misplacement of the head. One error thus led him into another. The erroneous posterior uterine obliquity forced him to represent the left side as presenting in the very commencement of labour in an ordinary case of first cranial position with the occiput looking to the left. I do not see how the difficulty, Smellie's yielding to which gave rise to error, can be avoided, except by assuming that the ordinary view as to the axis of the pregnant uterus is correct.

At the same point where Smellie stumbled, Nægele also fell into error, but in an opposite direction. In his classical essay on the mechanism of birth, describing the first position of the fœtal head, he represents it as presenting at the brim of the pelvis, which it has not yet fully entered, more obliquely than when it has entered it, or as having at the earliest stage its perpendicular axis more inclined anteriorly to the axis of the brim; and in this way he accounts for his allegation that the right ear can generally be felt at this time without difficulty behind the pubic bone.* Here a

* See the work of H. F. Nægele, "Die Lehre vom Mechanismus der Geburt." Mainz, 1838, S. 12.

remark may be made similar to that applied to Smellie's drawing; namely, that the head could not be so placed unless the uterus had an anterior obliquity, an obliquity opposite in direction to that figured by Smellie and described by Schatz; an obliquity quite incompatible with Nægele's own description in his work on the female pelvis;* or unless the child maintained an unnatural and undescribed left lateral flexion of its head.

The now generally entertained views, that the axis of the uterus coincides with the axis of the brim of the pelvis, and that the fœtal head presents at the brim directly,† have at least the merit of evading such obvious and adverse criticism as the figure of Smellie, and the expressed opinions of Schultze, Schatz, and of Nægele, are liable to be subjected to.

The great authority of Nægele was long sufficient to give currency to his statement that the head of the fœtus, as it passed through the brim of the pelvis, had its vertical axis in a position of anterior obliquity to the plane of the brim, an obliquity which is appropriately designated the Nægele obliquity, in order to distinguish it from other obliquities at the same situation. The great argument against this view, and the only one having a final character, is, that it is not an accurate description of what takes place; but in addition, it has been argued against it that it is impossible to find a mechanism to account for it. Stoltz's attempt to explain its occurrence by mere lateral flexibility of the neck of the child is insufficient, because it affords no explanation why the lateral flexion is towards the posterior shoulder; but the now alleged posterior obliquity of the uterus, as regards the axis of the brim, affords a solution which Nægele did not foresee when he described this obliquity as present and increasing with the increasing height of the head in or above the true pelvis. If, adopting the kind of nomenclature introduced by Barnes, we describe a curve of the natural promontory, produced at the brim of the pelvis by the posterior obliquity of the uterus, then this curve, representing a deflection of the axis to the extent of about ten degrees, can be easily made to account for the alleged Nægele obliquity during the first half of the passage of the child's head through the ligament-

* F. C. Nægele. "Das Weibliche Becken." Carlsruhe, 1825.

† See my "Researches in Obstetrics," p. 334, &c.

ous pelvis. For, if we suppose with Schatz that the whole power of labour acts in an oblique line nearly corresponding to that of the axis of the uterus, or inclined still more posteriorly, then there will always be a tendency of the anterior half of the head, or of that which is nearer the concavity of the curvature of the passage, to descend first, and so produce the Nægele obliquity, if there be uniform resistance to the advance of all parts of the head. But, as the occurrence of Nægele's obliquity is now very generally denied, any mechanism which accounts for it derives little or no support of its own accuracy from the circumstance of its doing so.

Still another difficulty in the way of admitting the presence of the curve of the natural promontory as the natural or ordinary condition is worthy of consideration. It is justly held that in natural labour the advance of the head through the brim of the pelvis is impeded only by friction and imperfect dilatation or dilatability of the soft parts; but, if this curve of the natural promontory exists, a new and considerable difficulty is introduced, namely, the difference between driving a body through a curved and a straight passage—a new difficulty which it appears to me unreasonable to admit. And this is not all; for this addition of difficulty is not overcome and passed when the child's head has traversed the curve, but lasts during most of the process of the birth of the child. If this curve exists, the axis of the genital passage, regarded in the antero-posterior vertical plane, has the shape of a Roman S; its first or upper curve, the curve of the natural promontory, having its concavity looking backwards; its second and universally recognised curve having its concavity looking forwards. I believe we are nearer the truth when adopting the view at present generally entertained, that, in the antero-posterior vertical plane, the genital passage has ordinarily only one curve, having the concavity of its axis looking forwards.

Direct therapeutical bearings of this matter are evident and important both in natural and morbid parturition. Certain attitudes of the body, by increasing or diminishing the flexion of the iliac beams upon the sacrum, a movement which I have elsewhere described as nutation of the sacrum,* may alter not only the dimensions of certain parts, but also the relations of the axis of the

* Researches in Obstetrics, p. 148.

pelvic brim to the axis of the uterus, or to the direction of the resultant of the forces of labour. In an elaborate paper Schultze* has attempted to show that similar results may be produced by flexion and extension of the spine. This author assumes that the lower lumbar vertebræ govern the uterine axis, and that the latter is normally inclined posteriorly to the plane of the pelvic brim. He therefore recommends that when difficulty arises at the brim, the spine should be flexed so as to bring the axes of the uterus and of the brim, if possible, into coincidence; and if we admit his assumptions, there can be no doubt as to the justice of his conclusion. For practical application, however, the proper treatment may be stated in such a way as to offend no theory as to axes of brim or of uterus, or so as to stand good whatever view is held on these points. When, before labour, or while the fœtal head is still mobile above the brim, it is placed with its sagittal suture not traversing the centre of the brim, but lying anterior to it (as Smellie figures), then it will during early labour be pressed, with a loss of force, against the pubes, not directly into the brim. It will then be worth while to try whether flexion of the spine, by putting the woman into the attitude assumed in stooping forward, will correct the direction of the head [which I consider an unnatural direction]. If it corrects it, the sagittal suture will be observed to leave the neighbourhood of the pubes and approach or reach the middle of the plane of the brim. Again, if the uterine axis, or the resultant of the forces of labour, has this posterior obliquity to the axis of the brim, then, in the first half of its course through the ligamentous pelvis, the fœtal head may be expected to show the Nægele obliquity—that is, its half lying in the anterior half of the pelvis will be lower than that in the posterior as regards the plane of the pelvic brim, being pushed down with greater force; and it will be well worth while to try whether or not flexion of the spine will correct this direction of the head [which I consider an unnatural direction].

II. The second curvature of the pelvis, which I proceed to describe, is, like the former, situated at the brim of the pelvis; but

* Jenaische Zeitschrift für Medicin und Natur-Wissenschaft, iii. Band. S. 272.

of its frequent existence there can be no doubt whatever. Its presence is indicated by the deflexion of the uterus from the mesial line to the right or to the left; and it is well known to be observed at all times—that is, before, during, and after pregnancy; but as this paper is concerned only with dynamical matters, this deflexion or deviation is interesting only as observed during labour. On the direction of this deflexion, to right or to left, I have no remarks to make, but I may refer the student first to the recent paper on this subject by Winkler,* and then to the earlier observations of Spiegelberg † on this uterine position during labour. For my present purpose it is more important to have some idea of the amount of deflexion which occurs. With a view to ascertain it, however imperfectly, I examined a series of cases which I found to present this condition. I did not, in all of these cases, make out whether or not the deflexion persisted during uterine action; but I ascertained that it did so in some of them. I hope to make further observations on this point, but such an inquiry is not essential to my present purpose, it being sufficient to know that the deviation does generally persist during the so-called erection of the uterus in a pain.

I proceeded as follows. Having the pregnant woman lying flat on her back, I made out the position of the uterus by feeling its outline with my hands; this manipulation shortly induced a pain which made the uterine form more distinct than previously; and then I could observe the outline mark the projection of the direction of the axis on the skin, and notice its just incidence on the outline of the fundus. Then I measured off, as on a plane, the angle between the projection of the axis and the vertical line joining the ensiform cartilage and the symphysis pubis. I did not try to have guidance from feeling the uterine angles and the parts attached thereto, as Winkler has done in similar circumstances, because I thought that such guidance would not ensure greater approach to accuracy in the measurements I wished to make with a view to purely dynamical considerations.

This angle I found in five cases to be 8, 10, 11, 14, 15 degrees respectively, or on an average about 10 degrees. The problem now

* Jenaische Zeitschrift, iv. Band. S. 522. 1868.

† Monatsschrift für Geburtskunde, xxix. Band. S. 92. 1867.

to be solved, is to make out from this angle on the surface of the spheroid what is the corresponding deflexion of the axis of the spheroid; and since the angle, as measured low down on the surface of the abdomen lies in a plane nearly parallel to that in which the axis of the uterus is deflected from the antero-posterior mesial plane, the deflexion of the axis may be regarded as nearly identical in amount with the angle measured on the surface. It is probable that this angle of deviation of the axis of the uterus from the axis of the brim of the pelvis has important physiological and practical bearings; but as yet little has been made out regarding them. It has been looked upon as affording some explanation of the alleged comparative frequency of laceration of the cervix on the left side in ordinary labour.* But the most interesting application of it is to assist in accounting for the production of face cases.† It has been shown how, under certain conditions, and supposing a right lateral deviation of the uterus, the part of the head on the left side of the brim—that is, the seat of the concavity of the curvature, will have a greater tendency to descend—that is, to be more powerfully pushed downwards than the part on the right side of the brim. Of this there can be no doubt; and the probability of this being a true theory or explanation of face cases is highly increased by remarking the apt manner in which other things, known in regard to face presentations, adapt themselves to it.

Another ingenious dynamical theory of face presentation has been started by Schatz. He states it as follows:—"When the uterus alone is in action, or when there is also acting uniform resistance around by the walls of the pelvis, a cranial presentation always occurs, if the occipital foramen of the fœtal head at the time of the first more important shortening of the long axis of the uterus lies backwards from this towards the back of the fœtus, but a face presentation, if it deviates forwards from this towards the breast side of the fœtus. With the co-operation of non-uniform resistance by the walls of the pelvis, cranial presentation is produced if the occurring positive or negative distance of the great occipital foramen towards the back of the fœtus from the

* Edinburgh Medical Journal, June 1871, p. 1061.

† Edinburgh Medical Journal, May 1870.

long axis of the uterus multiplied into the positive or negative difference of resistance by the walls of the pelvis, is greater on the posterior side of the fœtus than the product of the same factors on the breast side. In the opposite circumstances face presentation is produced."* To all this ingenious theorising there can be no objection if the conditions are assumed. But the two chief premises are merely assumed; they are not shown to occur; they are not shown to be more likely to occur in face presentation cases than in others. Under these circumstances, I submit that there can be no hesitation in preferring the formerly described theory of face cases, where the corresponding assumptions or premises are not mere assumptions, but well-known facts; I refer to the occasional lateral deviation of the uterus, the occasional dolichocephalous condition of the head, and the greater liability of cases of the second or right occipital position to be transformed into face cases than of the first or left occipital position.

III. The last curve of the developed genital passage which falls to be considered is the most extensive and the best known. It is the great curve in the antero-posterior vertical plane, which begins about the middle of the third bone of the sacrum and extends through the outlet of the ligamentous pelvis to the outlet from the soft parts. Its length may be greatly diminished by rupture of the perineum, and still more if the sphincter ani is torn through. It forms a curve, whose amount of bending varies from about 60 to about 150 degrees.

In connection with this curve fall to be studied the synclitic and allied movements of the fœtal head during its progress, to which Kueneke has recently directed attention, and which have been so carefully discussed at home and abroad,† that it is unnecessary to re-enter upon them here.

In connection with this curve have also to be studied the development of the lower part of the genital passage, the greater development posteriorly where the force is particularly or more strongly applied, than anteriorly where there is little more than

* Der Geburt's Mechanismus der Kopfendlagen, S. 72.

† See Edinburgh Medical Journal, June 1870, and the American Journal of the Medical Sciences, October 1870, &c.

counter-pressure, or pressure against a fixed wall, and that chiefly during the temporary abeyance of the power of parturition. There is to be noted, also, in connection with this curve, the inevitable tendency of the force of labour, not merely to distend the perineum, but also to rupture it centrally, to force the presenting part through it; a tendency the study of which, apart from other considerations, leaves no possible doubt as to the expediency of the practice of supporting the perineum, a practice which can be demonstrated to favour the maintenance of its entirety.

A novel practice, founded upon what I regard as a misapprehension of the conditions of this curvature, has been recently much dwelt upon by Professor Schultze of Jena.* The practice has for its object to facilitate and promote the advance of the child after its head has reached the floor of the pelvis. It is proposed to effect this by extension of the spine, with a view to which a hard pillow is to be placed beneath the loins as the woman lies on her back. The extension of the spine he believes to increase the posterior obliquity of the axis of the uterus, and therefore of the force of labour as exerted in this part. By the change supposed to be thus effected in the direction of the axis of the uterus, the axis of the force of labour is brought more nearly to the direction of the axis of the outlet of the pelvis, whereby there is supposed to be produced a diminution of the otherwise necessary loss of power arising from the change of direction of the passage at this part. Schultze alleges that he has found this extension of the spine to be useful in practice. If this utility is confirmed and ascertained, nothing, of course, can be said against it. But for the enforcement of his recommendation of this practice, it is evident that he trusts chiefly to theoretical arguments; and, therefore, I proceed to examine them, and believe I shall show that they are fallacious. Before doing so, it is worth while to point out that the attitude recommended by Schultze is a very unnatural one, and that a woman straining in labour advanced to the stage at present under consideration naturally assumes an attitude nearly opposite to that implied by extension of the spine, an attitude of some degree of flexion, an attitude which, keeping in view the relaxed state of the sacro-

* See Jenaische Zeitschrift für Medicin, &c. Band iii., 1867, and Lehrbuch für der Hebammenkunst, 1870.

sciatic ligaments, may be accompanied by some degree of enlargement of the outlet by the posterior nutation of the apex of the sacrum.

To Schultze's theory of the facilitation of the latter part of the second stage of labour by extension of the spine several objections may be made. First, it is inconsistent with his views as to the facilitation of the entry of the fœtal head into the brim of the pelvis by flexion of the spine. That view is based upon the assumption that the child's head enters the brim of the pelvis so as pretty nearly to occupy it and have a nearly vertical axis in the axis of the brim. If this be true of the fœtal head at the brim, it will be true of it during its course, *mutatis mutandis*, and it will be true of that part of the body which occupies the brim when the child's head is pressing on the perineum. It will be impossible, therefore, by any change of the axis of the uterus to bring the line of the labour force to bear upon the perineum in the direction of a straight line as Schultze represents it. Second, the upper cylindrical solid portion of the ligamentous pelvis, having a length of at least an inch and a half, has a well-determined axis with which must correspond the axis of any body fully occupying it, if the body is of uniform consistence,—conditions with which the fœtus nearly complies. If this be the case, the direction of the force of labour will follow the same axis, and no change of its direction above the brim of the pelvis, however produced, can have any effect upon its direction in any part below the brim of the pelvis. Third, Schultze forgets that his practice is intended to produce or increase posterior obliquity of the axis of the uterus to the brim, to increase the supposed curve of the natural promontory, and that every additional degree of that curve necessarily produces additional loss of power. The more, then, he extends the spine he will diminish the power of labour available at the outlet of the pelvis, instead of increasing it, as he expects. Fourth, if Schultze's * views, as illustrated by his diagrams, are correct, a dangerous amount and direction of force would be brought to bear upon the perineum, a structure whose integrity is already sufficiently imperilled by a force whose direction is gradually changed as the fœtus passes through the lower half of the ligamentous pelvis.

* Lehrbuch der Hebammenkunst, fig. liii.

Before concluding the consideration of the great curve of the genital passage in the anteroposterior vertical mesial plane, it is necessary to point out an important difficulty introduced into its study by the change in the condition of the ovum when passing through it, as compared with the ordinary condition of the ovum when passing the pelvic brim. Hitherto I have spoken on the assumption that the ordinary view of the action of the power of labour holds good at all parts of the course of the child. This view is, that the power is uniformly applied by the concave surface of the approximately spheroidal uterus to the uniform surface of the approximately spheroidal ovum, in a direction corresponding to the axis of the uterus and of the developed genital passage. Now, this view is probably nearly correct so long as the membranes are unruptured, or while no special part of the fœtus impinges on the uterus so as to injure its approximately spheroidal form, and provided no part of the fœtus impinges on the passage so as to cause special friction or obstruction at the part impinging. But while the great anteroposterior vertical curvature of the genital passage is being permeated, this view is no longer tenable, although even then it may, in a confessedly inexact way, be advantageously kept in mind, if other more exact conditions are not stated. While the curve is being described, the membranes are generally ruptured and the waters more or less completely discharged; and consequently the fœtus is in a variety of places impinging on and changing the form of the propelling uterus, and meeting with frictional obstruction in the passage at special points more than at others. These changes introduce an amount of complication of the problem which damages greatly the value of such considerations as I have above adduced, and I see no means at present of overcoming it and of arriving at exactness, though there is probably no insuperable difficulty in the matter. Another element of confusion is introduced by the want of uniformity which exists in the composition of the fœtus as a mechanical body. It is especially to be noted that it contains a longitudinally-placed elastic beam of connected vertebræ, which lies nearer the surface of the mass at one side than at the other.

The ovum or fœtus, in its passage through the developed genital canal, is subjected in various circumstances to various rotations on

some more or less longitudinally directed axis. It is also subject, in various circumstances, to various revolutions or sinuous deflexions, in which its long axis moves through portions of curves which are measured by corresponding angles. On these curves and their influence I have made a few remarks while feeling deeply their imperfection and the need of much further observation and research. The student who has followed the argument in this paper will have observed the resort to inferences when direct observations would have been preferable. This remark applies to every subject discussed in it; and while it is to be greatly regretted that such is the case, it is at the same time not to be forgotten that no method of making direct and exact observations has hitherto been discovered. The adoption of the homalographic method is surrounded with difficulties, not only in the method itself, but also in the procuring of subjects on which to use it; and while results obtained by it would be of great interest and importance, it is evident that they would not be complete or sufficient, for they can never be other than observations on parts in the repose of death, not in the turgescence and action of life. Until very recently, all our knowledge of the force of labour was on a like imperfect footing; but already ingenuity has suggested a means of basing this subject on exact observations, and Schatz has availed himself of these means, and greatly assisted us to arrive at results which we regard as probably the most important hitherto achieved in obstetric science. Till some ingenuity has succeeded in devising means of making like exact observations to settle the points discussed in this paper, we must be content to do our best to reach the truth by reasoning on what we do know more or less exactly. And it should be remembered that, by this method, we may reach the greatest assurance, if not certainty. A boy, playing with his dissected puzzle-map, may be certain that a county is rightly placed if it fits exactly into an entire hole formed of the conterminous boundaries of surrounding counties, especially if it also fits in nowhere else. So a theory which suits itself to all, or is in opposition to none, of numerous known conterminous conditions, may be, provisionally at least, assumed to be correct, and such assumption of correctness will vary with the number and testing character of the conditions so humoured by the theory.

3. On a Method of Determining the Explosive Power of Gaseous Combinations. By James Dewar, Esq.

(*Abstract.*)

The author describes an apparatus by means of which the explosive power of gaseous combinations can easily be determined, and from this, by Bunsen process, the temperature may readily be calculated. The essential feature of the apparatus is the registration of the "compression volume" of a given initial volume of air, on which the gaseous explosive mixture has been allowed to act. As the duration of the pressure is all but instantaneous, the well-known formula

$$\frac{P_2}{P_1} = \left(\frac{V_1}{V_2}\right)^{1\cdot4}$$

may be employed to ascertain the final pressure, more especially as the sudden rebound prevents any great loss of heat. In order to test the apparatus many experiments were made with mixtures of hydrogen and oxygen, and the mean result arrived at was a condensation to one-fifth the original volume of air (the initial volume being measured at 30 in. bar), when pure electrolytic gas was employed. This is equivalent to a pressure of 9·5 atmospheres, and therefore agrees with Bunsen's previous determination. The author hopes to be able to execute a series of determinations under varying conditions of temperature and pressure.

4. Note on Sprengel's Mercurial Air-Pump. By James Dewar, Esq.

The ordinary Sprengel, requiring careful manipulation, and being apt to get out of order, has not yet become an essential piece of lecture apparatus as it ought to be. The author exhibited to the Society two modifications adapted to lecture illustration. In both instruments the mercury receptacle is made of iron, and instead of the india-rubber joint of the original, a well-ground iron stopcock is substituted, the portion of iron tube before the stopcock terminating in a Y-shaped piece bored out of the solid. In the one form the drop-tube is of glass, attached by means of marine

glue; in the other, of carefully made india-rubber tube four or five millimetres in thickness, of a very small uniform bore, made expressly for the purpose by the Edinburgh Rubber Company. The iron funnel-shaped receptacles are ground at the inner apex, so as to fit perfectly finely-ground iron tubes. By means of these tubes the preliminary exhaustions are made by a hand pump, and then they are withdrawn. This device saves a separate joint. The barometer tubes are attached to solid T-shaped pieces of iron tube, and between these pieces and the main tubes each has a small glass bulb. Both forms work for all practical purposes as well as glass, and suit admirably for Frankland's water analyses, and Graham's experiments, &c. They may be procured from Mr Cameron, philosophical instrument maker, South Bridge, Edinburgh.

5. Professor Alexander Dickson exhibited a large series of abnormal cones of *Pinus Pinaster* which were to form the subject of a future communication to the Society.

The following Gentleman was balloted for and admitted as a Fellow of the Society:—

Archibald Constable, Esq.

Monday, 4th March 1872.

Professor MACQUORN RANKINE, Vice-President, in the Chair.

The following Communications were read:—

1. On the Connection between Chemical Constitution and Physiological Action—*Continued.* On the Physiological Action of the Salts of Trimethylsulphin. By Prof. Crum Brown and Dr Thomas R. Fraser.

In the former parts of this investigation we studied the physiological action of the salts of a considerable number of ammonium

bases—that is, of the salts formed by the union of an ether with the *nitride* of one or more alcohol radicals. Thus—

$$\underset{\substack{\text{Trimethylamine}\\\text{(Nitride of Methyl).}}}{(CH_3)'_3N} + \underset{\text{Iodide of Methyl.}}{CH_3I} = \underset{\substack{\text{Iodide of Tetramethyl-}\\\text{ammonium.}}}{(CH_3)_4NI}$$

$$\underset{\substack{\text{Methylconia}\\\text{(Nitride of Methyl and }(C_8H_{14})''\text{).}}}{(C_8H_{14})''(CH_3)N} + \underset{\text{Iodide of Methyl.}}{CH_3I} = \underset{\text{Iodide of Dimethylconium.}}{(C_8H_{14})''(CH_3)_2NI}$$

$$\underset{\substack{\text{Strychnia}\\\text{(Nitride of }(C_{21}H_{22}NO_2)'''\text{).}}}{(C_{21}H_{22}NO_2)'''N} + \underset{\text{Iodide of Methyl.}}{CH_3I} = \underset{\text{Iodide of Methylstrychnium.}}{(C_{21}H_{22}NO_2)'''(CH_3)NI}$$

The examination of the physiological action of such salts proved that, while differing from one another in many respects, there are two points in which they agree—they all paralyse the end-organs of the motor nerves, and none of them possess that stimulating action of the spinal cord which we observe in such a substance as strychnia.

Some years ago Von Œfele discovered that the sulphide of ethyl forms a compound with the iodide of ethyl, exactly as the nitride of ethyl (triethylamine) does. To this new salt he gave the name of iodide of triethylsulphin, and from it obtained the hydrated oxide and various other compounds of triethylsulphin. The number of known salts of this type has been increased by Cahours and Dehn.

As there are two ways in which the salts of the ammonium bases may be represented,—1st, as molecular compounds of nitrides with ethers; and 2d, as compounds of pentad nitrogen,—so the salts of the sulphin bases may be represented, either, 1st, as molecular compounds of sulphides with ethers; or, 2d, as compounds of tetrad sulphur.

As our physiological observations had led us to prefer the second mode of representing the constitution of the salts of the ammonium bases, it appeared to us that it would be of interest to examine the physiological action of the salts of the sulphin bases. We have accordingly commenced with the simplest salts of this type, viz., the salts of trimethylsulphin, and have made a number of experiments with the iodide and the sulphate of that radical. The iodide was employed in the form of pure white crystals; the sulphate,

which is an excessively deliquescent salt, was employed in the form of an aqueous solution of known strength. We found that the action of the two salts was identical, the difference of dose being nearly proportional to the chemical equivalent. In the case of warm-blooded animals the symptoms observed were—increasing weakness of the voluntary muscles ending with fatal doses in asphyxia, considerable contraction of the pupils, and profuse salivation.

In the case of frogs complete paralysis of the voluntary muscles was produced, along with a remarkable stiffness of the muscles of the anterior part of the body. By experiments conducted exactly as described in former papers read before the Society, we proved that the paralysis of the voluntary muscles was caused by the destruction of the function of the motor end-organs, the nerve trunks and the muscular fibres being still active. In fact, the action of these salts is almost identical with that of the salts of tetramethyl-ammonium, as formerly described by us.

We intend to continue these investigations, and to extend them to the corresponding compounds of selenium and tellurium and to the remarkable series of salts derived from $Se(CH_3)_2Cl_2$ and $Te(CH_3)_2Cl_2$, such as $Se(CH_3)_2OHNO_3$, &c.

2. On the Mean Monthly Rainfall of Scotland. By Alexander Buchan.

So far as regards the annual amounts of the rainfall of Scotland, deduced from observations made at 296 different places, the chief point brought out is the enormous difference between the rainfall of the west and that of the east; the stations along the west coast showing such figures as 40, 45, and 54 inches, as compared with 24, 27, and 30 inches at stations on the east coast, not situated in the immediate neighbourhood of hills. When it is considered that the source of the rainfall is the prevailing south-westerly winds, it is evident that the comparative dryness of such districts as the south shore of the Firth of Forth is due to high land lying to the south-west, which drains the winds of a large portion of their moisture in their passage across them. On the other hand, in the West Highlands, where arms of the sea open in upon the land in all direc-

tions from south round to west, the case is that of a high district, with currents of moist air poured in upon it, and the consequence is, an enormous rainfall, amounting, for example, at Glencroe to 128 inches, and at the head of Lochlomond to 115 inches. Between these extremes the amount of the rainfall varies, the variations being dependent on the physical configuration of the surface.

The monthly average rainfall has been examined by the discussion of observations made at 126 places for long terms of years —the number of years varying from 10 to 60, and the whole averaging 21 years. Of the stations dealt with, 54 are on the west slope, and 72 on the east slope. The mean annual rainfall for the whole country, deduced from these averages, is 44 inches; for the eastern slope 38 inches, and for the western slope 50 inches,—amounts which are probably not far from the true averages of these different regions.

In December, the general average for the whole country is greatly above the average monthly fall; in May it falls to the minimum, after which it continues to increase till it again rises considerably above the monthly average in October, to fall again, however, to about the average in November. The curve of the rainfall of the east, as compared with that of the west, shows the wet and dry seasons to be less strongly marked in the east; or the departures from the monthly averages are larger in the west. Since, however, the curves closely resemble each other, the general causes bringing about the deposition of rain in the west and in the east are the same. But at all seasons the absolute amount of the rainfall is greater in the west than in the east.

The largest monthly rainfall takes place in *December* in the north-western and western districts, and in the mountainous districts of the interior; in *January*, in the south-west, the Ochil Hills, and east of Perthshire; whereas, at a number of places in the drier districts, *August* is the month of largest rainfall.

The month of least rainfall is *April*, in the south of Scotland, *May* in the north, and *June* in Orkney, Shetland, and Farö; and it is remarkable that these same months are the months of largest (or very large) rainfall in various extensive regions on the continent of Europe.

3. Note on the Strain-Function. By Professor Tait.

When the linear and vector function expressing a strain is self-conjugate the strain is pure. When it is not self-conjugate, it may be broken up into pure and rotational parts in various ways (analogous to the separation of a quaternion into the *sum* of a scalar and a vector part, or into the *product* of a tensor and a versor part), of which two are particularly noticeable. Denoting by a bar a self-conjugate function, we have thus either

$$\phi = \overline{\psi} + \mathrm{V}.\epsilon(\ ),$$
$$\phi = q\overline{\varpi}(\ )q^{-1}, \text{ or } \phi = \overline{\varpi}.q(\ )q^{-1},$$

where ϵ is a vector, and q a quaternion (which may obviously be regarded as a mere versor).

That this is possible is seen from the fact that ϕ involves nine independent constants, while $\overline{\psi}$ and $\overline{\varpi}$ each involve six, and ϵ and q each three. If ϕ' be the function conjugate to ϕ, we have

$$\phi' = \overline{\psi} - \mathrm{V}.\epsilon(\ )$$

so that

$$2\overline{\psi} = \phi + \phi'$$

and

$$2\,\mathrm{V}.\epsilon(\ ) = \phi - \phi'$$

which completely determine the first decomposition. This is, of course, perfectly well known in quaternions, but it does not seem to have been noticed as a theorem in the kinematics of strains that there is always one, and but one, mode of resolving a strain into the geometrical composition of the separate effects of (1) a *pure* strain, and (2) a rotation accompanied by uniform dilatation perpendicular to its axis, the dilatation being measured by (sec. $\theta - 1$) where θ is the angle of rotation.

In the second form (whose solution does not appear to have been attempted) we have

$$\phi = q\overline{\varpi}(\ )q^{-1},$$

where the pure strain precedes the rotation; and from this

$$\phi' = \overline{\varpi}.q^{-1}(\ )q,$$

or in the conjugate strain the rotation (reversed) is followed by the pure strain. From these

$$\begin{aligned}\phi'\phi &= \overline{\varpi}.q^{-1}(q\overline{\varpi}(\ )q^{-1})q \\ &= \overline{\varpi}^2,\end{aligned}$$

and ϖ is therefore to be found by the solution of a biquadratic equation, as in *Proc. R. S. E.*, 1870, p. 316. It is evident, indeed, from the identical equation

$$S . \sigma\phi'\phi\rho = S . \rho\phi'\phi\sigma$$

that the operator $\phi'\phi$ is self-conjugate.

In the same way

$$\phi\phi' (\ \ ) = q^{-1} \varpi^2 (q (\ \ ) q^{-1}) q$$

or

$$q (\phi\phi'\rho) q^{-1} = \varpi^2 (q\rho q^{-1}) = \phi'\phi (q\rho q^{-1})$$

which show the relations between $\phi\phi'$, $\phi'\phi$, and q.

To determine q we have

$$\phi\rho . q = q \varpi \rho$$

whatever be ρ, so that

$$S . Vq (\phi - \varpi) \rho = 0,$$

or

$$S . \rho (\phi' - \varpi) Vq = 0,$$

which gives

$$(\phi' - \varpi) Vq = 0.$$

The former equation gives evidently

$$Vq \parallel V . (\phi - \varpi) \alpha (\phi - \varpi) \beta$$

whatever be α and β; and the rest of the solution follows at once. A similar process gives us the solution when the rotation precedes the pure strain.

4. On the Motion of Rigid Solids in a Liquid circulating Irrotationally through Perforations in them or in any Fixed Solid.* By Sir William Thomson.

1. Let ψ, ϕ, ... be the values at time t, of generalised co-ordinates fully specifying the positions of any number of solids movable through space occupied by a perfect liquid destitute of rotational motion, and not acted on by any force which could produce

* The title and first part (§§ 1 ... 13) are new, The remainder (§§ 14, 15) was communicated to the Royal Society at the end of last December.—W. T. *September* 26, 1872.

it. Some or all of these solids being perforated, let χ, χ', χ'', &c., be the quantities of liquid which from any era of reckoning, up to the time t, have traversed the several apertures. According to an extension of Lagrange's general equations of motion, used in Vol. I. of Thomson and Tait's "Natural Philosophy," §§ 331...336, proved in §§ 329, 331 of the German translation of that volume, and to be farther developed in the second English edition now in the press, we may use these quantities χ, χ', ... as if they were co-ordinates so far as concerns the equations of motion. Thus, although the position of any part of the fluid is not only not explicitly specified, but is actually indeterminate, when ψ, φ, ... χ, χ', ... are all given, we may regard χ, χ' ...... as specifying all that it is necessary for us to take into account regarding the motion of the liquid, in forming the equations of motion of the solids; so that if ξ, η, ..., and Ψ, Φ ... denote the generalised components of momentum and of force [Thomson and Tait, § 313 (*a*) (*b*)] relatively to ψ, φ ..., and if κ, κ', ... K, K′ ... denote corresponding elements relatively to χ, χ'..., we have (Hamiltonian form of Lagrange's general equations)

$$\left.\begin{aligned} \frac{d\xi}{dt} + \frac{\mathfrak{d}T}{d\psi} = \Psi, \quad \frac{d\eta}{dt} + \frac{\mathfrak{d}T}{d\varphi} = \Phi \ldots\ldots \\ \frac{d\kappa}{dt} + \frac{\mathfrak{d}T}{d\chi} = K, \quad \frac{d\kappa'}{dt} + \frac{\mathfrak{d}T}{d\chi'} = K' \ldots\ldots \end{aligned}\right\} \quad . \quad . \quad (1),$$

where T denotes the whole kinetic energy of the system, and $\mathfrak{d}$ differentiation on the hypothesis of ξ, η, ... κ, κ' ... constant.

2. To illustrate the meaning of χ, K, κ, χ', ..., let B be one of the perforated solids, to be regarded generally as movable, draw an immaterial barrier surface Ω across the aperture to which they are related, and consider this barrier as fixed relatively to B. Let N denote the normal component velocity, relatively to B and Ω of the fluid at any point of Ω; and let $\iint d\sigma$ denote integration over the whole area of Ω: then

$$\iint N d\sigma = \dot{\chi} \quad . \quad . \quad . \quad (2);$$

and

$$\chi = \int dt \iint N d\sigma \quad . \quad . \quad . \quad (3),$$

which is a symbolical expression of the definition of χ. To the

surface of fluid coinciding with Ω at any instant, let pressure be applied of constant value K per unit of area, over the whole area; and at the same time let force (or force and couple) be applied to B equal and opposite to the resultant of this pressure supposed for a moment to act on a rigid material surface Ω rigidly connected with B. The "motive" (that is to say, system of forces) consisting of the pressure K on the fluid surface, and force and couple B as just defined, constitutes the generalised component force corresponding to χ [Thomson and Tait, § 313 (*b*)]; for it does no work upon any motion of B or other bodies of the system if χ is kept constant; and if χ varies work is done at the rate

$$K\dot{\chi} \text{ per unit of time,}$$

whatever other motions or forces there may be in the system. Lastly, calling the density of the fluid unity, let κ denote "circulation"* [V. M. § 60 (*a*)]† of the fluid in any circuit crossing β once, and only once: it is this which constitutes the generalised component momentum relatively to χ [Thomson and Tait, § 313 (*e*)]; for (V. M. § 72) we have

$$\kappa = \int_0^t K dt. \quad . \quad . \quad . \quad (4),$$

if the system given at rest (or in any state of motion for which $\kappa = 0$) be acted on by the motive K during time t.‡

3. The kinetic energy T is, of course, necessarily a quadratic function of the generalised momentum-components, $\xi, \eta, \ldots \kappa, \kappa' \ldots$; with coefficients generally functions of $\psi, \varphi, \ldots$, but necessarily independent of $\chi, \chi', \ldots$. In consequence of this peculiarity it is convenient to put

$$T = Q(\xi - \alpha\kappa - \alpha'\kappa' - \&c., \eta - \beta\kappa - \beta'\kappa' - \&c., \ldots) + \mathfrak{Q}(\kappa, \kappa', \ldots) \ (5),$$

* Or $\int F ds$ if F denote the tangential component of the absolute velocity of the fluid at any point of the circuit, and $\int ds$ line integration once round the circuit.

† References distinguished by the initials V. M. are to the part already published of the author's paper on Vortex Motion. (*Transactions of the Royal Society af Edinburgh*, 1867–8 *and* 1868–9.)

‡ The general limitation, for impulsive action, that the displacements effected during it are infinitely small, is not necessary in this case. Compare § 5 (11), below.

where Q, $\mathfrak{Q}$ denote two quadratic functions. This we may clearly do, because, if i be the number of the variables $\xi, \eta, \cdots$, and j the number of $\kappa, \kappa' \ldots$; the whole number of coefficients in the single quadratic function expressing τ is $\frac{(i+j)(i+j+1)}{2}$, which is equal to the whole number of the coefficients $\frac{i(i+1)}{2} + \frac{j(j+1)}{2}$ of the two quadratic functions, together with the $i\,j$ available quantities $\alpha, \beta, \ldots \alpha', \beta', \ldots, \ldots$

4. The meaning of the quantities $\alpha, \beta, \ldots \alpha', \ldots$ thus introduced is evident when we remember that

$$\frac{d\mathrm{T}}{d\xi} = \dot{\psi}, \frac{d\mathrm{T}}{d\eta} = \dot{\phi}, \ldots \frac{d\mathrm{T}}{d\kappa} = \dot{\chi}, \frac{d\mathrm{T}}{d\kappa'} = \dot{\chi}', \ldots \qquad (6).$$

For; differentiating (5), and using these, we find

$$\dot{\psi} = \frac{d\mathrm{Q}}{d\xi}, \dot{\phi} = \frac{d\mathrm{Q}}{d\eta}, \ldots \qquad (7),$$

and using these latter,

$$\dot{\chi} = \frac{d\mathfrak{Q}}{d\kappa} - \alpha\dot{\psi} - \beta\dot{\phi} - \&c., \dot{\chi}' = \frac{d\mathfrak{Q}}{d\kappa'} - \alpha'\dot{\psi} - \beta'\dot{\phi} - \&c., \ldots \qquad (8).$$

Equations (8) show that $-\alpha\dot{\psi}$, $-\beta\dot{\phi}$, $-\alpha'\dot{\psi}$, &c., are the contributions to the flux across Ω, Ω', &c., given by the separate velocity-components of the solids. And (7) show that to prevent the solids from being set in motion when impulses $\kappa, \kappa', \cdots$ are applied to the liquid at the barrier surfaces, we must apply to them impulses expressed by the equations

$$\xi = \alpha\kappa + \alpha'\kappa' + \&c., \eta = \beta\kappa + \beta'\kappa' + \&c., \ldots \qquad (9).$$

5. To form the equations of motion, we have, in the first place,

$$\frac{\delta\mathrm{T}}{d\chi} = 0, \frac{\delta\mathrm{T}}{d\chi'} = 0 \ldots \qquad (10),$$

and therefore, by (1),

$$\frac{d\kappa}{dt} = \mathrm{K}, \frac{d\kappa'}{dt} = \mathrm{K}', \ldots \qquad (11);$$

which show that the acceleration of κ, under the influence of K, follows simply the law of acceleration of a mass under the influence of a force. Again (for the motions of the solids), let

$$\xi_0 = \xi - \alpha\kappa - \alpha'\kappa' - \&c., \ \eta_0 = \eta - \beta\kappa - \beta'\kappa' - \&c., \ldots \qquad (12);$$

and let $\frac{\mathfrak{D}Q}{d\psi}$, &c., denote variations of Q on the hypothesis of ξ_0, η_0, ... each constant.

We have from (5), remembering that $\frac{\mathfrak{d}T}{d\psi}$, &c., denote variations of T, on the hypothesis of ξ, η, ... κ, κ', ... constant,

$$\frac{\mathfrak{d}T}{d\psi} = \frac{\mathfrak{D}Q}{d\psi} - \frac{dQ}{d\xi}\left(\kappa\frac{d\alpha}{d\psi} + \kappa'\frac{d\alpha'}{d\psi} + \ldots\right) - \frac{dQ}{d\eta}\left(\kappa\frac{d\beta}{d\psi} + \kappa'\frac{d\beta'}{d\psi} + \&c.\right) - \&c. + \frac{\mathfrak{d}\mathfrak{Q}}{d\psi},$$

or, by (7)

$$\frac{\mathfrak{d}T}{d\psi} = \frac{\mathfrak{D}Q}{d\psi} - \dot{\psi}\left(\kappa\frac{d\alpha}{d\psi} + \kappa'\frac{d\alpha}{d\psi} + \&c.\right) - \phi\left(\kappa\frac{d\beta}{d\psi} + \kappa'\frac{d\beta'}{d\psi} + \&c.\right) - \&c. + \frac{\mathfrak{d}\mathfrak{Q}}{d\psi} \ldots \quad . \quad (13).$$

Hence by (1)

$$\frac{d\xi}{dt} + \frac{\mathfrak{D}Q}{d\psi} - \dot{\psi}\left(\kappa\frac{d\alpha}{d\psi} + \kappa'\frac{d\alpha'}{d\psi} + \&c.\right) - \phi\left(\kappa\frac{d\beta}{d\psi} + \kappa'\frac{d\beta'}{d\psi'} + \&c.\right) - \&c. + \frac{\mathfrak{d}\mathfrak{Q}}{d\psi} = \Psi \ldots \quad (14).$$

Now, remark that, according to the notation of (12), ξ_0, η_0,... are the momentum-components of the solids due to their own motion alone, without cyclic motion of the liquid; and therefore eliminate ξ, η,... by (12) from (14). Thus we find

$$\begin{aligned}\frac{d\xi_0}{dt} + \frac{\mathfrak{D}Q}{d\psi} + \alpha\frac{d\kappa}{dt} + \alpha'\frac{d\kappa'}{dt} + \&c. &+ \phi\left\{\kappa\left(\frac{d\alpha}{d\phi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\phi} - \frac{d\beta'}{d\psi}\right) + \&c.\right\} \\ &+ \theta\left\{\kappa\left(\frac{d\alpha}{d\theta} - \frac{d\gamma}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\phi} - \frac{d\gamma'}{d\psi}\right) + \&c.\right\} \\ &+ \quad \&c. \qquad\qquad = \Psi - \frac{\mathfrak{d}\mathfrak{Q}}{d\psi} \ldots (15),\end{aligned}$$

which, with the corresponding equation for ξ_0, &c., and with (11) for κ, κ', &c., are the desired equations of motion.

6. The hypothetical mode of application of K, K',... (§ 1) is impossible, and every other (such as the influence of gravity on a real liquid at different temperatures in different parts) is impossible for our ideal "liquid," that is to say, a homogeneous incompressible perfect fluid. Hence we have K = 0, K' = 0, and from (11)

conclude that $\kappa, \kappa', \ldots$ are constants. [They are sometimes called the "cyclic constants (V. M. §§ 62...64)]. The equations of motion (15) thus become simply

$$\frac{d\xi_0}{dt} + \frac{\mathfrak{D}Q}{d\psi} + \dot{\varphi}\left\{\kappa\left(\frac{d\alpha}{d\varphi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\varphi} - \frac{d\beta'}{d\psi}\right) + \ldots\right\}$$
$$+ \dot{\theta}\left\{\kappa\left(\frac{d\alpha}{d\theta} - \frac{d\gamma}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\theta} - \frac{d\beta'}{d\psi}\right) + \cdots\right\}$$
$$+ \&c. \qquad = \Psi - \frac{\mathfrak{d}\mathfrak{Q}}{d\psi} \ldots (16),$$

with corresponding equations for η_0, ζ_0, and with the following relations from (7), between ξ_0, $\eta_0 \cdots$ and $\dot{\psi}$, $\dot{\varphi}, \cdots$

$$\frac{dQ}{d\xi_0} = \dot{\psi}, \quad \frac{dQ}{d\eta_0} = \dot{\varphi}, \quad \frac{dQ}{d\zeta_0} = \dot{\theta}, \ \&c. \quad . \quad . \quad (17).$$

7. Let

$$\kappa\left(\frac{d\alpha}{d\varphi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\varphi} - \frac{d\beta'}{d\psi}\right) + \&c., \text{ be denoted by } \{\varphi, \psi\} \quad . \quad (18),$$

so that we have

$$\{\varphi, \psi\} = -\{\psi, \varphi\} \quad . \quad . \quad . \quad . \quad (19).$$

These quantities $\{\varphi, \psi\}$, $\{\theta, \psi\}$, &c., linear functions of the cyclic constants, with coefficients depending on the configuration of the system, are to be generally regarded simply as given functions of the co-ordinates ψ, φ, θ, ... : and the equations of motion are

$$\left.\begin{array}{l} \dfrac{d\xi_0}{dt} + \dfrac{\mathfrak{D}Q}{d\psi} + \{\varphi, \psi\}\dot{\varphi} + \{\theta, \psi\}\dot{\theta} + \&c. = \Psi - \dfrac{\mathfrak{d}\mathfrak{Q}}{d\psi} \\ \dfrac{d\eta_0}{dt} + \dfrac{\mathfrak{D}Q}{d\varphi} - \{\varphi, \psi\}\dot{\psi} + \{\theta, \varphi\}\dot{\theta} + \&c. = \Theta - \dfrac{\mathfrak{d}\mathfrak{Q}}{d\theta} \\ \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{array}\right\} \quad . \quad . \quad (20).$$

In these (being of the Hamiltonian form) Q is regarded as a quadratic function of ξ_0, η_0, $\zeta_0 \cdots$ with its coefficients functions of ψ, φ, θ, &c.; and $\mathfrak{D}$ applied to it indicates variations of these coefficients. If now we eliminate ξ_0, η_0, $\zeta_0 \cdots$ from Q by the linear equations, of which (17) is an abbreviated expression, and so have Q expressed as a quadratic function of $\dot{\psi}$, $\dot{\varphi}$, $\dot{\theta}, \cdots$, with its coefficients functions of ψ, φ, θ, &c.; and if we denote by $\frac{dQ}{d\varphi}$, $\frac{dQ}{d\psi}$, &c., variations of Q depending on variations of these co-

efficients; and by $\frac{dQ}{d\dot{\psi}}$, $\frac{dQ}{d\dot{\phi}}$, &c., variations of Q depending on variations of $\dot{\psi}$, $\dot{\phi}$, &c.; we have [compare Thomson and Tait, § 329 (13) and (15)]

$$\left.\begin{aligned} &\xi_0 = \frac{dQ}{d\dot{\psi}}, \quad \eta_0 = \frac{dQ}{d\dot{\psi}}, \ldots\ldots\ldots\ldots \\ \text{and} \qquad &\frac{\mathfrak{d}Q}{d\psi} = -\frac{dQ}{d\psi}, \quad \frac{\mathfrak{d}Q}{d\varphi} = -\frac{dQ}{d\varphi}, \ldots\ldots \end{aligned}\right\} \quad . \quad (21);$$

and the equations of motion become

$$\left.\begin{aligned} &\frac{d}{dt}\frac{dQ}{d\dot{\psi}} - \frac{dQ}{d\psi} + \{\varphi, \psi\}\dot{\phi} + \{\theta, \psi\}\dot{\theta} + \ldots = \Psi - \frac{\mathfrak{d}\mathfrak{Q}}{d\psi} \\ &\frac{d}{dt}\frac{dQ}{d\dot{\phi}} - \frac{dQ}{d\varphi} - \{\varphi, \psi\}\dot{\psi} + \{\theta, \varphi\}\dot{\theta} + \ldots = \Phi - \frac{\mathfrak{d}\mathfrak{Q}}{d\varphi} \\ &\frac{d}{dt}\frac{dQ}{d\dot{\theta}} - \frac{dQ}{d\theta} - \{\theta, \psi\}\dot{\psi} - \{\theta, \varphi\}\dot{\phi} + \ldots = \Theta - \frac{\mathfrak{d}\mathfrak{Q}}{d\theta} \\ &\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}\right\} (22).$$

The first members here are of Lagrange's form, with the remarkable addition of the terms involving the velocities simply (in multiplication with the cyclic constants) depending on the cyclic fluid motion. The last terms of the second members contain traces of their Hamiltonian origin in the symbols $\frac{\mathfrak{d}}{d\psi}$, $\frac{\mathfrak{d}}{d\varphi}$, ...

8. As a first application of these equations, let $\dot{\psi} = 0$, $\dot{\phi} = 0$, $\dot{\theta} = 0$, ... This makes $\xi_0 = 0$, $\eta_0 = 0$..., and therefore also $Q = 0$; and the equations of motion (16), (now equations of equilibrium of the solids under the influence of applied forces Ψ, Φ, &c., balancing the fluid pressure due to the polycyclic motion κ, κ',...), become

$$\Psi = \frac{\mathfrak{d}\mathfrak{Q}}{d\psi}, \ \Phi = \frac{\mathfrak{d}\mathfrak{Q}}{d\varphi}, \text{ \&c.}, \quad . \quad . \quad . \quad (23);$$

a result which a direct application of the principle of energy renders obvious (the augmentation of the whole energy produced by an infinitesimal displacement, $\delta\psi$, is $\frac{\mathfrak{d}\mathfrak{Q}}{d\psi}\delta\psi$, and $\Psi\delta\psi$ is the work done by the applied forces). It is proved in §§ 724 ... 730 of a volume of collected papers on electricity and magnetism soon to be

published, that $\frac{\delta \mathfrak{Q}}{d\psi}$, $\frac{\delta \mathfrak{Q}}{d\varphi}$, &c., are the components of the forces experienced by bodies of perfect diamagnetic inductive capacity placed in the magnetic field analogous * to the supposed cyclic irrotational motion. Hence the motive influence of the cyclic motion of the liquid upon the solids in equilibrium is equal and opposite to that of magnetism in the magnetic analogue.

This is proposition II. of the paper "On the Forces experienced by Solids immersed in a Moving Liquid," which relates to the forces required to keep the movable solids at rest. The present investigation shows Prop. II. of that article to be false. Compare "Reprint," § 740.

9. Equations (16) for the case of a single perforated movable solid undisturbed by others, agree substantially with equations (6) and (14) of my communication † to the Royal Society of Edinburgh of February 1871. The $\xi_0, \eta_0, \ldots$ of the present article correspond to the $\frac{dT}{du}$, $\frac{dT}{dv}$, &c., of the former; the $\xi, \eta, \ldots$ mean the same in both. The equations now demonstrated constitute an extension of the theory not readily discovered or proved by that simple consideration of the principle of momentum, and moment of momentum, on which alone was founded the investigation of my former article.

10. Going back to the analytical definition of $\mathfrak{Q}$ in § 3 (5), we see that when none of the movable solids is perforated, this configurational function is equal to the whole kinetic energy (E), which the polycyclic motion would have were there no movable solid, diminished by the energy (W) which would be given up were the liquid, which on this supposition flows through the space of the movable solid or solids, suddenly rigidified and brought to rest. Putting then

$$\mathfrak{Q} = E - W \quad . \quad . \quad . \quad (24),$$

and remarking that E is independent of the co-ordinates of the movable solids, we may put $-W$ in place of $\mathfrak{Q}$ in the equations of motion, which, for this slight modification, need not be written

* Proposition I. of article on "The Forces experienced by Solids immersed in a Moving Liquid" (*Proceedings R. S. E., February* 1870, reprinted in Volume of Electric and Magnetic papers, §§ 733 ... 740).

† See Proceedings R. S. E., Session 1870–71, or reprint in Philosophical Magazine, Nov. 1871.

out again. W might be directly defined as the whole quantity of work required to remove the movable solids, each to an infinite distance from any other solid having a perforation with circulation through it; and, with this definition, $-$W may be put for $\mathfrak{Q}$ in the equations of motion without exclusion of cases in which there is circulation through apertures in movable solids.

11. I conclude with a very simple case, the subject of my communication to the Royal Society of last December, in which the result was given without proof. Let there be only one moving body, and it spherical; let the perforated solid or solids be reduced to an infinitely fine immovable rigid curve or group of curves (endless, of course, that is, either finite and closed, or infinite), and let there be no other fixed solid. The rigid curve or curves will be called the "core" or "cores," as their part is simply that of core for the cyclic or polycyclic motion. In this case it is convenient to take for ψ, φ, θ, the rectangular co-ordinates (x, y, z) of the centre of the movable globe. Then, because the cores, being infinitely fine, offer no obstruction to the motion of the liquid, making way for the globe moving through it, we have

$$Q = \tfrac{1}{2}m(\dot{x}^2 + \dot{y}^2 + \dot{z}^2) \quad . \quad . \quad (25),$$

where m denotes the mass of the globe, together with half that of its bulk of the fluid. Hence

$$\left.\begin{array}{l} \dfrac{dQ}{dx} = 0,\ \dfrac{dQ}{dy} = 0,\ \dfrac{dQ}{dz} = 0, \\ \text{and} \\ \xi_0\left(= \dfrac{dQ}{d\dot{x}}\right) = m\dot{x},\ \eta_0 = m\dot{y},\ \zeta_0 = m\dot{z} \end{array}\right\} \quad . \quad (26).$$

A farther great simplification occurs, because in the present case $\alpha d\psi + \beta d\varphi + \ldots$, or, as we now have it, $\alpha dx + \beta dy + \gamma dz$, is a complete differential.* To prove this, let V be the velocity-potential at any point (a, b, c) due to the motion of the globe, irrespectively of any cyclic motion of the liquid. We have

$$V = \tfrac{1}{2}r^3\left(\dot{x}\frac{d}{dx} + \dot{y}\frac{d}{dy} + \dot{z}\frac{d}{dz}\right)\frac{1}{D},$$

* Which means that if the globe, after any motion whatever, great or small, comes again to a position in which it has been before, the integral quantity of liquid which this motion has caused to cross any fixed area is zero.

where r denotes the radius of the globe, and $D = \{(x-a)^2 + (y-b)^2 + (z-c)^2\}^{\frac{1}{2}}$. Hence if N denote the component velocity of the liquid at (a, b, c) in any direction λ, μ, ν, we have

$$N = \left(\dot{x}\frac{d}{dx} + \dot{y}\frac{d}{dy} + \dot{z}\frac{d}{dz}\right) F\,(x, y, z, a, b, c), \qquad (27),$$

where

$$F\,(x, y, z, a, b, c,) = \tfrac{1}{2}r^3\left(\lambda\frac{d}{da} + \mu\frac{d}{db} + \nu\frac{d}{dc}\right)\frac{1}{D}\,.$$

Let now (a, b, c) be any point of the barrier surface Ω (§ 2), and λ, μ, ν, the direction cosines of the normal. By (2) of § 2 we see that the part of $\dot{\chi}$ due to the motion of the globe is $\iint N d\sigma$, or, by (26),

$$\left(\dot{x}\frac{d}{dx} + \dot{y}\frac{d}{dy} + \dot{z}\frac{d}{dz}\right)\iint F\,(x, y, z, a, b, c)\, d\sigma \qquad (28).$$

Hence, putting

$$\iint F\,(x, y, z, a, b, c)\, d\sigma = U\,,$$

we see by (8) of § 4, that

$$\alpha = -\frac{dU}{dx},\ \beta = -\frac{dU}{dy},\ \gamma = -\frac{dU}{dz} \quad . \qquad (29).$$

Hence, with the notation of § 7 (18) for $x, y, \ldots$ instead of $\psi, \varphi, \ldots$

$$\{y, z\} = 0,\ \{z, x\} = 0,\ \{x, y\} = 0.$$

By this and (25) the equations of motion (22), with (24), become simply

$$m\frac{d^2x}{dt^2} = X + \frac{\partial W}{dx},\ m\frac{d^2y}{dt^2} = Y + \frac{\partial W}{dy},\ m\frac{d^2z}{dt^2} = Z + \frac{\partial W}{dz} \quad (30).$$

These equations express that the globe moves as a material particle of mass m, with the forces (X, Y, Z) expressly applied to it, would move in a "field of force," having W for potential.

12. The value of W is of course easily found by aid of spherical harmonics, from the velocity potential, P, of the polycyclic motion which would exist were the globe removed, and which we must suppose known: and in working it out (small print below) it is readily seen that if, for the hypothetical undisturbed motion, q denote the fluid velocity at the point really occupied by the centre of the rigid globe, we have

$$W = \tfrac{1}{2}\mu q^2 + w \quad . \quad . \qquad (31),$$

where μ denotes one and a half times the volume of the globe, and w denotes the kinetic energy of what we may call the internal motion of the liquid occupying for an instant in the undisturbed motion the space of the rigid globe in the real system. To define w, remark that the harmonic analysis proves the velocity of the centre of inertia of an irrotationally moving liquid globe to be equal to q, the velocity of the liquid at its centre;* and consider the velocity of any part of the liquid sphere, relatively to a rigid body moving with the velocity q. The kinetic energy of this relative motion is what is denoted by w. Remark also that if, by mutual forces between its parts, the liquid globe were suddenly rigidified, the velocity of the whole would be equal to q; and that $\frac{1}{2}mq^2$ is the work given up by the rigidified globe and surrounding liquid when the globe is suddenly brought to rest, being the same as the work required to start the globe with velocity q from rest in a motionless liquid.

Let $P + \psi$ be the velocity potential at (x, y, z) in the actual motion of the liquid when the rigid globe is fixed. Let a be the radius of the globe, r distance of (x, y, z) from its centre, and $\iint d\sigma$ integration over its surface. At any point of the surface of the instantaneous liquid globe, the component velocity perpendicular to the spherical surface in the undisturbed motion is $\left(\frac{dP}{dr}\right)_{r=a}$; and hence the impulsive pressure on the spherical surface required to change the velocity potential of the external liquid from P to $P+\psi$, being $-\psi$, undoes an amount of work equal to

$$\iint d\sigma \psi \, . \, \tfrac{1}{2} \frac{dP}{dr} ,$$

in reducing the normal component from that value to zero. On the other hand, the internal velocity-potential is reduced from P to zero, and the work undone in this process is

$$\iint d\sigma P \, . \, \tfrac{1}{2} \frac{dP}{dr} .$$

* This follows immediately from the proposition (Thomson and Tait's "Natural Philosophy," § 496) that any function V, satisfying Laplace's equation $\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2}$ throughout a spherical space has for its mean value through this space its value at the centre. For $\frac{dP}{dx}$ satisfies Laplace's equation.

Hence

$$W = \tfrac{1}{2}\iint d\sigma\,(P+\psi)\frac{dP}{dr}, \quad . \quad . \qquad (32).$$

The condition that with velocity-potential $P+\psi$ there is no flow perpendicular to the spherical surface, gives

$$\left(\frac{dP}{dr}+\frac{d\psi}{dr}\right)_{r=a} = 0 \quad . \quad . \quad . \qquad (33).$$

Now let

$$\left.\begin{aligned} P &= P_0 + P_1\frac{r}{a} + \ldots\ldots + P\left(\frac{r}{a}\right)^i + \&c. \\ \psi &= \Psi_1\left(\frac{a}{r}\right)^2 + \ldots\ldots + \Psi_i\left(\frac{a}{r}\right)^{i+1} + \&c. \end{aligned}\right\} . \qquad (34),$$

be the spherical harmonic developments of P and ψ relatively to the centre of the rigid globe as origin, the former necessarily convergent throughout the largest spherical space which can be described from this point as centre without enclosing any part of the core; the latter necessarily convergent throughout space external to the sphere. By (33) we have

$$\Psi_i = \frac{i}{i+1}P_i \quad . \quad . \quad . \quad . \qquad (35).$$

Hence (32) gives

$$W = \tfrac{1}{2}\iint d\sigma\left(\Sigma\frac{2i+1}{i+1}P_i\right)\left(\Sigma iP_i\right),$$

which, by

$$\iint d\sigma P_i P_{i'} = 0,$$

becomes

$$W = \frac{1}{2a}\,\Sigma\frac{i(2i+1)}{i+1}\iint d\sigma P_i^2 \quad . \quad . \quad . \qquad (36).$$

Now, remarking that a solid spherical harmonic of the first degree may be any linear function of x, y, z, put

$$P_1\frac{r}{a} = Ax + By + Cz \quad . \quad . \quad . \qquad (37),$$

which gives

$$q^2 = A^2 + B^2 + C^2,$$

and

$$\frac{1}{a}\iint d\sigma P_1^2 = (A^2 + B^2 + C^2)\,.\,\frac{a}{3}\,.\iint d\sigma = q^2 \times \text{volume of globe} = \frac{2}{3}\mu q^2.$$

Hence by (36

$$W = \tfrac{1}{2}\mu q^2 + \tfrac{1}{2}\iint d\sigma\left(\frac{2\,.\,5}{3}P_2^2 + \frac{3\,.\,7}{4}P_3^2 + \ldots\right). \qquad (38);$$

and, therefore, by comparison with (31),

$$w = \tfrac{1}{2}\iint d\sigma\left(\frac{2\,.\,5}{3}P_2^2 + \frac{3\,.\,7}{4}P_3^2 + \ldots\right) \quad . \qquad (39).$$

13. When the radius of the globe is infinitely small,

$$W = \tfrac{1}{2}\mu q^2 \quad . \quad . \quad . \quad . \quad (40),$$

where μ denotes one and a half times the volume of the globule, and q the undisturbed velocity of the fluid in its neighbourhood. This corresponds to the formula which I gave twenty-five years ago for the force experienced by a small sphere (whether of ferromagnetic or diamagnetic non-crystalline substance) in virtue of the inductive influence which it experiences in a magnetic field.*

14. By taking an infinite straight line for the core a simple but very important example is afforded. In this case, the undisturbed motion of the fluid is in circles having their centres in the core (or axis, as we may now call it), and their planes perpendicular to it. As is well known, the velocity of irrotational revolution round a straight axis is inversely proportional to distance from the axis. Hence the potential function W for the force experienced by an infinitesimal solid sphere in the fluid is inversely as the square of the distance of its centre from the axis, and therefore the force is inversely as the cube of the distance, and is towards the nearest point of the axis. Hence, when the globule moves in a plane perpendicular to the axis, it describes one or other of the forms of Cotesian spirals†. If it be projected obliquely to the axis, the component velocity parallel to the axis will remain constant, and the other component will be unaffected by that one; so that the projection of the globule on the plane perpendicular to the axis will always describe the same Cotesian spiral as would be described were there no motion parallel to the axis. If the globule be left to itself in any position it will commence moving towards the axis as if attracted by a force varying inversely as the cube of the distance. It is remarkable that it traverses at right angles an increasing liquid current without any applied force to prevent it

* "On the Forces Experienced by Small Spheres under Magnetic Influence, and some of the Phenomena presented by Diamagnetic Substances" (*Cambridge and Dublin Mathematical Journal, May* 1847); and "Remarks on the Forces experienced by Inductively Magnetised Ferromagnetic or Diamagnetic Non-crystalline Substances" (*Phil. Mag. October* 1850). Reprint of Papers on Electrostatics and Magnetism, §§ 634–668. Macmillan, 1872.

† Tait and Steele's "Dynamics of a Particle," § 149 (15).

from being (as we might erroneously at first sight expect it to be) carried sideways with the augmented stream. A properly trained dynamical intelligence would at once perceive that the constancy of moment of momentum round the axis requires the globule to move directly towards it.

15. Suppose now the globule to be of the same density as the liquid. If (being infinitely small) it is projected in the direction and with the velocity of the liquid's motion, it will move round the axis in the same circle with the liquid; but this motion would be unstable [and the neglected term w (39) adds to the instability]. Compare Tait and Steele's "Dynamics of a Particle," § 149 (15), Species IV., case A = 0 and AB finite; also limiting variety between Species I. and Species V. The globule will describe the same circle in the opposite direction if projected with the same velocity opposite to that of the fluid. If the globule be projected either in the direction of the liquid's motion or opposite to it, with a velocity less than that of the liquid, it will move along the Cotesian spiral (Species I. of Tait and Steele), from apse to centre in a finite time, with an infinite number of turns. If it be projected in either of those directions with a velocity greater by v than that of the liquid, it will move along the Cotesian spiral (Species V. of Tait and Steele), from apse to asymptote. Its velocity along the asymptote, at an infinite distance from the axis, will be

$$\sqrt{v\left(v+\frac{\kappa}{\pi a}\right)},$$

and the distance of the asymptote from the axis will be

$$a\frac{v+\frac{\kappa}{2\pi a}}{\sqrt{v\left(v+\frac{\kappa}{\pi a}\right)}},$$

where a denotes the distance of the apse from the axis, and $\frac{\kappa}{2\pi a}$ the velocity of the liquid at that distance from the axis. If the globule be projected from any point in the direction of any straight line whose shortest distance from the axis is p, it will be drawn into the vortex or escape from it, according as the component velo-

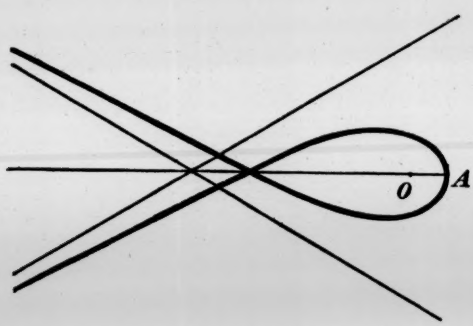
city in the plane perpendicular to the axis is less or greater than $\frac{\kappa}{2\pi p}$. It is to be remarked that in every case in which the globule is drawn in to the axis (except the extreme one in which its velocity is infinitely little less than that of the fluid, and its spiral path infinitely nearly perpendicular to the radius vector), the spiral by which it approaches, although it has always an infinite number of convolutions, is of finite length; and therefore, of course, the time taken to reach the axis is finite. Considering, for simplicity, motion in a plane perpendicular to the axis; at any point infinitely distant from the axis, let the globule be projected with a velocity v along a line passing at distance p on either side of the axis. Then if τ denote the velocity of the fluid at distance unity from the axis $\left[\text{which is equal to } \frac{\kappa}{2\pi}\right]$, and if we put

$$n^2 = 1 - \frac{\tau^2}{v^2p^2} \quad . \quad . \quad . \quad . \quad (41),$$

the polar equation of the path is

$$r = \frac{np}{\cos n\theta} \quad . \quad . \quad . \quad . \quad (42).$$

Hence the nearest approach to the axis attained by the globule is np, and the whole change of direction which it experiences is $\pi\left(\frac{1}{n} - 1\right)$. The case of $\frac{1}{n} = 2{\cdot}3$ is represented in the annexed diagram, copied from Tait and Steele's book [§ 149 (15), Species V.].



Monday, 18*th March* 1872.

PROFESSOR KELLAND, Vice-President, in the Chair.

The following Communications were read :—

1. On the Extraction of the Square Root of a Matrix of the Third Order. By Professor Cayley.

Professor Tait has considered the question of finding the square root of a strain, or what is the same thing, that of a matrix of the third order—

$$\begin{pmatrix} a, & b, & c \\ d, & e, & f \\ g, & h, & i \end{pmatrix}.$$

A mode of doing this is indicated in my "Memoir on the Theory of Matrices" (*Phil. Trans.*, 1858, pp. 17–37), and it is interesting to work out the solution.

The notation and method will be understood from the simple case of a matrix of the second order. I write

$$(x_1,\ y_1) \;=\; \begin{pmatrix} a, & b \\ c, & d \end{pmatrix} (x,\ y)\,,$$

to denote the two equations, $x_1 = ax + by$, $y_1 = cx + dy$. This being so, putting

$$(x_2,\ y_2) \;=\; \begin{pmatrix} a, & b \\ c, & d \end{pmatrix} (x_1,\ y_1), \;=\; \begin{pmatrix} a, & b \\ c, & d \end{pmatrix}^2 (x,\ y),$$

we arrive at the value of the squared matrix, viz.,

$$\begin{pmatrix} a, & b \\ c, & d \end{pmatrix}^2 \;=\; \begin{pmatrix} a^2 + bc, & b(a+d) \\ c(a+d), & d^2 + bc \end{pmatrix},$$

and we have similarly the third, fourth, and higher powers of a matrix. The zero power is the matrix unity, $= \begin{pmatrix} 1, & 0 \\ 0, & 1 \end{pmatrix}$.

The zero matrix is $\begin{pmatrix} 0, & 0 \\ 0, & 0 \end{pmatrix}$, and when a matrix is put $= 0$, this means that it is a matrix of the last-mentioned form.

Consider the matrix $\mathrm{M} = \begin{pmatrix} a, & b \\ c, & d \end{pmatrix}$; write down the equation,

$$\begin{vmatrix} a - \mathrm{M}, & b \\ c & , d - \mathrm{M} \end{vmatrix} = 0,$$

where the function on the left hand is a determinant, M being therein regarded in the first instance as a quantity, viz., this equation is

$$\mathrm{M}^2 - (a + d)\mathrm{M} + (ad - bc)\mathrm{M}^0 = 0;$$

and then substituting for M^2, M, M^0, their expressions as matrices, this equation is identically true, viz., it stands for the four identities—

$$\begin{aligned} a^2 + bc - (a + d)a + ad - bc &= 0, \\ b(a + d) - (a + d)b &= 0, \\ c(a + d) - (a + d)c &= 0, \\ d^2 + bc - (a + d)d + ad - bc &= 0, \end{aligned}$$

and the like property holds for a matrix of any order.

To extract the square root of the matrix $\mathrm{M} = \begin{pmatrix} a, & b \\ c, & d \end{pmatrix}$; in other words, to find a matrix $\mathrm{L} = \begin{pmatrix} \mathrm{a}, & \mathrm{b} \\ \mathrm{c}, & \mathrm{d} \end{pmatrix}$ such that $\mathrm{L}^2 = \mathrm{M}$; that is

$$\begin{pmatrix} \mathrm{a}^2 + \mathrm{bc}, & \mathrm{b}(\mathrm{a} + \mathrm{d}) \\ \mathrm{c}(\mathrm{a} + \mathrm{d}), & \mathrm{d}^2 + \mathrm{bc} \end{pmatrix} = \begin{pmatrix} a, & b \\ c, & d \end{pmatrix},$$

(four equations for the determination of a, b, c, d):—

The solution is as follows: write

$$\begin{vmatrix} a - \mathrm{M}, & b \\ c & , d - \mathrm{M} \end{vmatrix} = \mathrm{M}^2 - p\mathrm{M} + q,$$

(q is here written for $q\mathrm{M}^0$, and so in other cases); and similarly

$$\begin{vmatrix} \mathrm{a} - \mathrm{L}, & \mathrm{b} \\ \mathrm{c} & , \mathrm{d} - \mathrm{L} \end{vmatrix} = \mathrm{L}^2 - \mathrm{pL} + \mathrm{q},$$

then we have

$$\begin{aligned} &\mathrm{M}^2 - p\mathrm{M} + q = 0, \\ &\mathrm{L}^2 - \mathrm{pL} + \mathrm{q} = 0, \\ &\mathrm{L}^2 = \mathrm{M}; \end{aligned}$$

and from these equations we may express L as a linear function of M, M^0, with coefficients depending on p, q; and also determine the unknown quantities p, q in terms of p, q.

We, in fact, have

$$\mathrm{L} = \frac{1}{\mathrm{p}}(\mathrm{M} + \mathrm{q});$$

Also this gives $(\mathrm{M} + \mathrm{q})^2 - \mathrm{p}^2\mathrm{M} = 0$, that is

$$\mathrm{M}^2 - (\mathrm{p}^2 - 2\mathrm{q})\,\mathrm{M} + \mathrm{q}^2 = 0\,,$$

which must agree with

$$\mathrm{M}^2 - p\mathrm{M} + q = 0;$$

consequently,

$$\mathrm{p}^2 - 2\mathrm{q} = p,\ \mathrm{q}^2 = q\,,$$

that is,

$$\mathrm{q} = \sqrt{q},\ \mathrm{p} = \sqrt{p + 2\sqrt{q}}\,,$$

and then,

$$\mathrm{L} = \frac{1}{\mathrm{p}}(\mathrm{M} + \mathrm{q})\,,$$

which is the required solution; viz., this signifies

$$\mathrm{L} = \begin{pmatrix} \dfrac{a + \mathrm{q}}{\mathrm{p}}, & \dfrac{b}{\mathrm{p}} \\ \dfrac{c}{\mathrm{p}}, & \dfrac{d + \mathrm{q}}{\mathrm{p}} \end{pmatrix},$$

where p, q have the above-mentioned values—a result which can be at once verified. Observe that there are in all 4 solutions, but these correspond in pairs of solutions, differing only in their sign; the number of distinct solutions is taken to be = 2.

Passing now to the case of a matrix of the third order,

$$\mathrm{M} = \begin{pmatrix} a, & b, & c \\ d, & e, & f \\ g, & h, & i \end{pmatrix},$$

let the expanded value of the determinant

$$\begin{vmatrix} a - \mathrm{M}, & b, & c \\ d\ , & e - \mathrm{M}, & f \\ g\ , & h\ , & i - \mathrm{M} \end{vmatrix} \quad \text{be} = -(\mathrm{M}^3 - p\mathrm{M}^2 + q\mathrm{M} - r);$$

and let the required square root be

$$\mathrm{L} = \begin{pmatrix} \mathrm{a}, & \mathrm{b}, & \mathrm{c} \\ \mathrm{d}, & \mathrm{e}, & \mathrm{f} \\ \mathrm{g}, & \mathrm{h}, & \mathrm{i} \end{pmatrix}$$

and p, q, r, have the like significations in regard to L. Then from the equations—

$$\begin{aligned} &M^3 - pM^2 + qM - r = 0\,, \\ &L^3 - \mathrm{p}L^2 + \mathrm{q}L - \mathrm{r} = 0\,, \\ &L^2 = M\,, \end{aligned}$$

we can express L as a linear function of M^2, M, M^0, with coefficients depending on p, q, r; and obtain expressions for p, q, r, in terms of p, q, r.

We have

$$L\,(M + \mathrm{q}) = \mathrm{p}M + \mathrm{r}\,,$$

that is,

$$L = \frac{\mathrm{p}M + \mathrm{r}}{M + \mathrm{q}},\ = \mathrm{p} + \frac{\mathrm{r} - \mathrm{pq}}{M + \mathrm{q}}.$$

But we have

$$M^3 - pM^2 + qM - r = (M + \mathrm{q})\left(M^2 + \theta M + \varphi + \frac{\omega}{M + \mathrm{q}}\right)$$

where

$$\begin{aligned} -\theta &= \mathrm{q} + p\,, \\ \varphi &= \mathrm{q}^2 + \mathrm{q}p + q\,, \\ -\omega &= \mathrm{q}^3 + \mathrm{q}p^2 + \mathrm{q}q + r\,, \end{aligned}$$

and thence

$$L = \frac{\mathrm{pq} - \mathrm{r}}{\omega}\,(M^2 + \theta M + \varphi) + \mathrm{p}\,,$$

that is, $L = xM^2 + yM + z$, where x, y, z are given functions of p, q, r.

To determine these, observe that

$$\sqrt{M}(M + \mathrm{q}) = \mathrm{p}M + \mathrm{r}\,,$$

that is

$$M^3 - (\mathrm{p}^2 - 2\mathrm{q})M^2 + (\mathrm{q}^2 - 2\mathrm{pr})M - \mathrm{r}^2 = 0\,,$$

which must agree with

$$M^3 - pM^2 + qM - r = 0\,,$$

or we have

$$\mathrm{p}^2 - 2\mathrm{q} = p,\ \mathrm{q}^2 - 2\mathrm{pr} = q,\ \mathrm{r}^2 = r\,,$$

whence

$$\mathrm{r} = \sqrt{r}\,,$$

$$(\mathrm{q}^2 - q)^2 = 4\,(2\mathrm{q} + p)r\,,$$

$$\mathrm{p} = \frac{\mathrm{q} - q}{2\mathrm{r}}\,,$$

which are the required values; there being in all eight solutions, but these correspond in pairs of solutions of opposite sign, so that the number of independent solutions is = 4. The form of the result agrees in a remarkable manner with that obtained by Professor Tait on totally different principles (*ante*, p. 316).

I annex a further investigation, starting from the assumption that the solution is $\sqrt{M} = xM^2 + yM + z$; viz., writing for shortness—

$$M^2 = \left(\begin{array}{ccc} a', & b', & c' \\ d', & e', & f' \\ g', & h', & i' \end{array}\right),$$

then the solution is

$$\sqrt{M} = \left(\begin{array}{lll} xa' + ya + z, & xb' + yb \quad , & xc' + yc \\ xd' + yd \quad , & xe' + ye + z, & xf' + yf \\ xg' + yg \quad , & xh' + yh \quad , & i + yi + z \end{array}\right)$$

where observe that only a, e, i contain z; and that the differences e – i, i – a, a – e are independent of z. We ought to have

$$\begin{array}{l|l|l} a^2 + cg + bd = a & b(a+e) + ch = b & d(a+e) + fg = d, \\ e^2 + db + fh = e & f(e+i) + dc = f & h(e+i) + gb = h, \\ i^2 + hf + cg = i & g(i+a) + hd = g & c(i+a) + bf = c, \end{array}$$

viz., these nine equations should be satisfied by a common set of values of x, y, z; or, what is the same thing, the whole system should be equivalent to the first triad of equations. To verify this, observe that we can from the first triad (by the linear elimination of z^2 and z) obtain an equation of the form $(x, y)^3 + x = 0$; say this is the equation $\Omega = 0$. In fact, multiplying by e – i, i – a, a – e and adding, the three equations give

$$\begin{aligned} (e-i)(i-a)(a-e) + fh\,(e-i) + gc\,(i-a) + bd\,(a-e) & \\ + \ a(e-i) + \ e(i-a) + \ i(a-e) &= 0, \end{aligned}$$

where the first line contains terms of the form $(x, y)^3$, the second line is linear and

$$= [a(e'-i') + e\,(i'-a') + i\,(a'-e')]x,$$

viz., this is

$$= [(e-i)(i-a)(a-e) + fh(e-i) + gc(i-a) + bd(a-e)]x.$$

The whole equation divides by the coefficient of x, and the result is $(x, y)^3 + x = 0$.

Now, from any one of the remaining six equations, together with two equations of the first triad, we can obtain the same result, $\Omega=0$. Thus, if the selected equation is $\mathrm{b}(\mathrm{a}+\mathrm{e})+\mathrm{ch}-b=0$, then from the first and second equations of the triad we have

$$(\mathrm{a}^2-\mathrm{e}^2)+\mathrm{cg}-\mathrm{fh}-(a-e)=0\,,$$

and thence

$$(\mathrm{a}-\mathrm{e})(b-\mathrm{ch})+\mathrm{b}(\mathrm{cg}-\mathrm{fh})-\mathrm{b}(a-e)=0\,.$$

There is here the linear term $b(\mathrm{a}-\mathrm{e})-\mathrm{b}(a-e)$, viz., this is

$$=[b(a'-e')-b'(a-e)]x\,,$$

which is

$$=[-(a-e)ch+b(cg-fh)]x\,.$$

The whole equation divides by the coefficient of x, and gives the foregoing equation, $\Omega=0$.

Thus the equations reduce themselves to the first triad: writing these under the form

$$\frac{1}{a}(\mathrm{a}^2+\mathrm{cg}+\mathrm{bd})=\frac{1}{e}(\mathrm{e}^2+\mathrm{bd}+\mathrm{fh})=\frac{1}{i}(\mathrm{i}^2+\mathrm{hf}+\mathrm{cg})=1\,,$$

then omitting the last equation $(=1)$, these are of the form $\mathrm{U}=\mathrm{V}=\mathrm{W}$, where U, V, W are homogeneous quadric functions of x, y, z; viz., treating these as co-ordinates they represent two quadric cones, having a common vertex, and intersecting in 4 lines: or we have 4 sets of values of the ratios $x:y:z$: or for x, y, z themselves 8 sets of values; but, as before, these correspond in pairs, and the number of distinct solutions is taken to be $=4$.

I return to the equation $\Omega=0$. This is found to be

$$\begin{vmatrix} (a-p)x-y, & bx & cx \\ dx & (e-p)x-y & fx \\ gx & hx & (i-p)x-y \end{vmatrix} - x = 0$$

$(p=a+e+i$ as before); or what is the same thing, the equation is

$$\begin{vmatrix} a-p-\dfrac{y}{x}, & b, & c \\ d & , e-p-\dfrac{y}{x}, & f \\ g & , h & , i-p-\dfrac{y}{x} \end{vmatrix} - \frac{1}{x^2} = 0\,.$$

I verify this by the former solution, as follows:—We have

$$x = \frac{\mathrm{pq} - \mathrm{r}}{\omega},\ y = \frac{\mathrm{pq} - \mathrm{r}}{\omega}\theta;\ \text{that is, } \frac{y}{x} = \theta,\ = -p - \mathrm{q}.$$

The equation thus becomes

$$\begin{vmatrix} a + \mathrm{q}, & b, & c \\ d & , e + \mathrm{q}, & f \\ g & , h & , i + \mathrm{q} \end{vmatrix} - \frac{\omega^2}{(\mathrm{pq} - \mathrm{r})^2} = 0,$$

that is

$$\mathrm{q}^3 + p\mathrm{q}^2 + q\mathrm{q} + r - \frac{\omega^2}{(\mathrm{pq} - \mathrm{r})^2} = 0.$$

But we have

$$-\omega = \mathrm{q}^3 + p\mathrm{q}^2 + q\mathrm{q} + r,$$

and the equation thus becomes

$$\mathrm{q}^3 + p\mathrm{q}^2 + q\mathrm{q} + r - (\mathrm{pq} - \mathrm{r})^2 = 0;$$

viz., substituting for p, q, r their values in terms of p, q, r, this is the identity,

$$\mathrm{q}^3 + \mathrm{q}^2(\mathrm{p}^2 - 2\mathrm{q}) + \mathrm{q}(\mathrm{q}^2 - 2\mathrm{pr}) + \mathrm{r}^2 - (\mathrm{pq} - \mathrm{r})^2 = 0.$$

An interesting case is where the given matrix M is unity; that is

$$\mathrm{M} = \begin{pmatrix} 1, & 0, & 0 \\ 0, & 1, & 0 \\ 0, & 0, & 1 \end{pmatrix}.$$

We have here $p = 3$, $q = 3$, $r = 1$; the equation in q is

$$\mathrm{q}^4 - 6\mathrm{q}^2 - 8\mathrm{q} - 3 = 0;$$

that is $(\mathrm{q} - 3)(\mathrm{q} + 1)^3 = 0$; viz., $\mathrm{q} = 3$ or $\mathrm{q} = -1$. Taking, as we may do, $\mathrm{r} = +1$, we have the two solutions $(\mathrm{p} = 3,\ \mathrm{q} = 3,\ \mathrm{r} = 1)$ and $(\mathrm{p} = -1,\ \mathrm{q} = -1,\ \mathrm{r} = 1)$.

For the first of these $\theta = -6$, $\phi = 21$, $\omega = -64$, $\mathrm{pq} - \mathrm{r} = 8$, and thence

$$\mathrm{L} = -\frac{1}{8}(\mathrm{M}^2 - 6\mathrm{M} + 21) + 3,\ = 1, \text{ on writing therein } \mathrm{M} = 1;$$

viz., we have L the matrix unity, a self-evident solution.

But for the second, $\theta = -2$, $\phi = 1$, $\omega = 0$, $\mathrm{pq} - \mathrm{r} = 0$, and the solution takes the form $\sqrt{\mathrm{M}} = \frac{0}{0}(\mathrm{M} - 1)^2 - 1$. There is, in

fact, a solution containing four arbitrary constants, given (with some misprints) in the "Memoir on Matrices," and which (for convenience changing the signs) is as follows:—

$$L = \left(\begin{vmatrix} \dfrac{-\alpha}{\alpha+\beta+\gamma}, & \dfrac{(\beta+\gamma)\frac{\lambda}{\mu}}{\alpha+\beta+\gamma}, & \dfrac{(\beta+\gamma)\frac{\lambda}{\nu}}{\alpha+\beta+\gamma} \\ \dfrac{(\gamma+\alpha)\frac{\mu}{\lambda}}{\alpha+\beta+\gamma}, & \dfrac{-\beta}{\alpha+\beta+\gamma}, & \dfrac{(\gamma+\alpha)\frac{\mu}{\nu}}{\alpha+\beta+\gamma} \\ \dfrac{(\alpha+\beta)\frac{\nu}{\lambda}}{\alpha+\beta+\gamma}, & \dfrac{(\alpha+\beta)\frac{\nu}{\mu}}{\alpha+\beta+\gamma}, & \dfrac{-\gamma}{\alpha+\beta+\gamma} \end{vmatrix} \right),$$

(or, what is the same thing, we may omit the denominators, assuming $\alpha+\beta+\gamma=1$); it is, in fact, easy to verify that this has for its square the matrix unity. Moreover, we have, as above, $p=-1$, $q=-1$, $r=1$.

2. Second Note on the Strain Function. By Prof. Tait.

3. Note on the Rate of Cooling at High Temperatures. By Professor Tait.

4. Notice of a Large Boulder in the Parish of Rattray, and County of Perth, having on one of its sides Cups and Grooves, apparently artificial. By D. Milne Home.

About a year ago, the Council of this Society appointed a Committee to make inquiry about boulders in Scotland.

The Committee intend to submit to the Council a general report of their proceedings, showing the progress made.

The object of the present notice is to give to the Society an account of one of the boulders reported to the Committee, as a specimen of the information which they have been obtaining.

The Rev. Mr Herdman, minister of Rattray, in Perthshire, sent to the Committee an answer to their circular, specifying the following boulders and standing stones in his parish:—

1*st*, A stone known from time immemorial as the *Standing Stone of Glenballoch.*

This boulder is angular, and rudely pyramided in form. Its entire height is 12 feet. At its base it is about 8 feet square; and half-way up, about 6 feet square. Its weight is estimated at about 25 tons.

It rests on what Mr Herdman describes as a firm, hard, dry, sandy, reddish yellow clay, called by the farmers of the district, till.

On one side of this stone, viz., that facing the glen, on the north bank of which it stands, there are cuttings or incisions, which Mr Herdman, and others skilled in archæology who have examined them, believe to be artificial. These incisions are of two kinds: *First*, hemispherical cavities, about twelve or thirteen in number; and *second*, grooves which on some points touch or run into these cavities.

2*d*, In another part of the same estate, viz., of Craighall, belonging to Colonel Clark Rattray, there is a spot known as "The Stannin' Stanes." This name occurs in the parish records, Mr Herdman says, so far back as 300 years. There was a small farm long known by the name of "Stannin' Stanes;" and about forty years ago, there were dwelling-houses at the place, forming a hamlet which bore the same name.

Though there is only one large stone at this place, Mr Herdman is of opinion that it once had companions. These have disappeared. They are probably in dykes and cattle sheds, not far off.

The stone which remains, is, in length above ground, about 5 feet, and is about 4 feet square. It is believed to be sunk in the ground 3 feet. Its weight is estimated at 8 or 9 tons. It stands upright.

3*d*, There is *a group of stones*, each containing about 7 cubic yards of rock, and each weighing, probably, about 14 tons, situated on the farm of Glenballoch, not far from the large stone first mentioned. Lines joining these 4 stones would form an irregular square. The intervals between the stones are from 9 to 12 feet. The stone at the south-west angle is higher than the others, reaching to a point 5 feet above the ground. The other three stones lie on their sides.

4th, There is *another group of stones*, five or six in number, *on Hatton Hill*, about 500 yards to the east of the hill top, and about 20 feet below its level. Each of these stones is on average about a cubic yard in solid content, and weighs about two tons.

Hatton Hill is at its top about 900 feet above the sea. The farm of Glenballoch, on which most of the other stones are, is about 750 feet above the sea.

To revert now to the stone first mentioned, the annexed woodcut will give an idea of its shape. The cups or cavities on its sides—which, however, are not well shown on the diagram—are from 2 to 3 inches in diameter, and from half an inch to one inch deep. The grooves are about half an inch deep and about half an inch wide.

The cup-shaped cavities were first noticed about fourteen or fifteen years ago, by the Rev. Mr Herdman, and were shown by him to Dr Wise, a well-known archæologist. At that time the part of the stone above the surface of the ground measured about 9½ feet

from the top, and in that part of the stone there were only five or six cups discernible; plaster casts of these, however, were taken and sent to the Society of Scottish Antiquaries. No doubt was entertained by those who then examined the stone and the casts, that these cup cavities were artificial and not natural.

About six years ago the late Sir James Simpson turned his attention to the subject of these antique and mysterious cuttings and sculpturings, and drew out a memoir on the subject, illustrated by numerous lithographs, which was published by the Society of Antiquaries.

Mr Herdman having heard of this inquiry, was induced to make a farther examination of the stone, and had some of the earth cleared away from its sides. He then discovered other hemispherical cavities sharper and more distinct than those in the higher and more exposed part of the stone, and which greater distinctness he naturally ascribed to the covering of earth by which they had been protected from the weather. He also on this occasion observed that there were grooves or ruts on the surface of the stone, in the parts which had been covered up, and which were prolonged into grooves on the upper part of the stone where they were more faint.

It will be seen from the diagram,—*first*, that on the middle of the stone and near the cups there are two long grooves, with a cross groove at two places; *second*, that at the right hand there is a zigzag groove; and *third*, that at the left hand there is a straight groove, running up vertically, but more faint than the others. The second and third of these grooves were only discovered lately, and in consequence of investigations for the Boulder Committee.

Whenever the discovery of these additional cups and grooves was made, Mr Herdman lost no time in sending an account of them to Sir James Simpson. But by this time his memoir had been printed; and the only notice which appears in that memoir of the Glenballoch Stone, is in the following terms, p. 15:—

"*Circle at Craighall, Perthshire.*—Cup excavations exist upon an erect stone standing at a megalithic circle behind Craighall House, Blairgowrie. The cups are five or six in number, and placed in a group near the foot of the stone."

The account is incorrect in several particulars. Instead of there being only five or six cups, there are thirteen or fourteen. The four vertical and three transverse grooves are not mentioned. There is no reason to suppose that a circle of stones ever existed here. In fact the rapid slope of the ground, where the boulder stands, would have prevented such a circle being made. Megalithic circles are always on a flat piece of land. Sir James Simpson was never at Glenballoch, as he told Mr Herdman himself shortly before his death.

Whilst to Mr Herdman belongs the merit of discovering these markings, the still greater merit belongs to him of having saved this boulder from the fate which has befallen several others in his parish, and hundreds, or probably thousands, equally curious throughout Scotland. The boulder stands within the precincts of a field which bears good crops, and as it was a considerable obstruction to farming operations, the tenant about six years ago was preparing to break it up, and the more especially as he was then in want of stones for a new farm-house. His intentions having become known, the Rev. Mr Herdman would have applied to the proprietor himself had he been at home, to save the boulder. But he was abroad; and so the factor was appealed to, and fortunately with success.

The tenant has several times since thrown out dark hints about the inconvenience to which he is exposed by the presence of this boulder in an arable field, and also by the occasional visits of the curious to examine it. He has recently spoken of the damage done to his "neeps" by Mr Herdman's excavations; and it was only after much persuasion that Mr Herdman obtained from him a promise in these words, "Weel, I'll lat the stane alane, if you dinna *howk* muckle mair about it." Notwithstanding this assurance, Mr Herdman thinks it might be as well that the Royal Society Committee should communicate with the proprietor, Col. Clark Rattray, and ask him to give strict orders for the preservation of the boulder.

These remarks apply to the Glenballoch stone only in its archæological relations. But it is probably also interesting geologically. Mr Herdman states that he has not much knowledge of rocks, and no experience in geological researches. Nevertheless, the facts

related by him suggest some questions of considerable importance. He has had the kindness to send chips of all the stones specified by him. Mr Herdman describes them as, in his opinion, a black coloured trap. But they appear to be all bits of micaceous schist. The prevailing rock in the parish of Rattray is a coarse red sandstone—probably Old Red Sandstone, containing thick beds of coarse conglomerate.

The nearest rocks of micaceous schist are in the hills to the north and west. How far off they are it is not stated, nor how much higher in level than Rattray parish. But it is pretty evident that all these boulders came from the hills, and by natural agency of some kind. The stone of Glenballoch, weighing as it does 25 tons, must have come in that way; and it is almost certain that it now occupies the spot and position on which it was originally placed. The other stones specified by Mr Herdman probably do not now occupy their original site and position, as they seem to have been set up for the purposes—whatever these were—for which they were wanted. Probably the group of stones near the top of Hatton Hill are in their original position, for they do not seem to be artificially arranged; and, moreover, it is not uncommon to find boulders in heaps near the tops of hills, as if these hills had somehow obstructed the farther progress of the agent (whatever that was), which transported the boulders, and caused it to discharge its cargo on or near the top of the hill.

Assuming, then, that the stone of Glenballoch is an erratic from some northern or westerly point, one question would be, What caused the transporting agent to drop it at the place where it now stands? Why should it not have been carried farther? Perhaps an examination of the country might suggest data to aid in the solution of this question.

The position of the boulder and its attitude appear to deserve attention, provided it can be correctly assumed that they were received by natural and not by human agency.

Mr Herdman states that the boulder stands in a field which slopes pretty rapidly down towards a stream, running through a narrow glen. This field seems to form one side of that glen, or small valley, through which, he says, there was formerly a pass much

frequented between Craighall and Banff; and "*balloch*" is a Celtic word for "*pass.*" How high above the bottom of the glen the boulder stands, Mr Herdman does not explain. The boulder, therefore, stands in rather a critical position; and considering its great weight, it does not seem likely that it could have been put into that position by human agency.

Then its attitude is singular, because boulders having a longer and shorter axis are generally and naturally found lying with their longer axis parallel with the ground; but this boulder has its longer axis vertical, and stands on a basis of only 8 feet square. If the present position and attitude are those it received when it fell from the agent which transported it, what was the nature of the agent which allowed it to fall, so as to take that attitude?

The two theories for the transport of such boulders are *land ice*, as by a glacier, and *floating ice*, as by an iceberg or ice floe. Whether the country between Rattray parish and the mountains to the north is of such a nature as to have allowed the formation of a glacier may be a question, but supposing it were, which of these two ice agents, glacier or floating ice, would have been most likely to cause this pear-shaped block to fall into the position and attitude which it occupies? This is a question as much for a mathematician as for a geologist to solve.

5. On the Fruiting of the Ipecacuan Plant (*Cephaelis Ipeçacuanha,* Rich.) in the Royal Botanic Garden. By Prof. Balfour.

The cultivation of the Ipecacuan plant in this country has received an impetus from the demand on the part of His Grace the Duke of Argyll, for a large supply of fresh plants for India. The object of the India office is to cultivate the plant extensively, and thus prevent the evils which might arise from scarcity of a drug which is so important in the treatment of dysentery. The risk of such an occurrence is due to the mode in which the plant is gathered in Brazil, and the want of care in preserving it. A similar fate threatens Ipecacuan as that which has occurred in the case of Cinchona.

The Secretary of State for India has, in the first place, endeavoured to introduce the plant from this country—leaving for after consideration the propriety of getting specimens sent direct from Rio Janeiro to India. The plants in this country have been supplied from various sources. The original specimen, cultivated by Sir William Hooker in Glasgow, came from Liege, and the Messrs Lawson have imported recently a quantity of specimens from Belgium and Germany. In the Royal Botanic Garden of Edinburgh we are indebted for specimens—first, to Sir William Hooker; and, secondly, to Dr Gunning of Palmeiras, Rio Janeiro. Sir Robert Christison has taken a warm interest in the subject, and has aided much in procuring specimens. Mr M'Nab found that by cutting the root of the original garden plant he could propagate it easily, and in this way he secured a large stock. He gave to the Botanical Society of Edinburgh a notice of his mode of cultivation. This account was printed for the India office, and copies of it were extensively distributed. The specimens from Rio Janeiro were treated in a similar manner.

The plants were sent to India in Wardian cases, sometimes under charge of gentlemen of the forest department going to India, and sometimes without any one in charge. The results have been very successful.

The Duke of Argyll has forwarded to me a report by Dr G. King, superintendent of the Botanic Garden, Calcutta, to whose care the cases were consigned.

From Dr G. King, *Superintendent, Botanic Garden, Calcutta, to the Secretary to the Government of Bengal.*

"I have the honour to report, for the information of Government, the arrival from England of five consignments of Ipecacuanha plants. Five of these consignments, consisting of a single case each, were brought out under the care of Messrs Walton, Whittall, Jellicoe, Ferrais, and Gamble, officers newly appointed to the Forest Department. The sixth, consisting of three closed Wardian cases, came as deck-baggage on board the Suez Canal steamer, 'City of Mecca,' under the special care of no one.

"As will be seen by the following tabular statement, the total number of plants despatched from England was 277. On arrival

in Calcutta 15 plants were found to be dead, and 36 in a sickly state, leaving a balance of 226 healthy.

	Healthy.	Sickly.	Dead.	Total.	
Brought by Mr Walton, .	12	...	...	12	120 Botanic Garden.
" Mr Jellicoe, .	...	26	4	30	
" Mr Ferrais, .	12	...	...	12	
" Mr Gamble, .	27	2	5	34	
" Mr Whittall, .	26	4	2	32	
Received *ex* 'City of Mecca,'	149	4	4	157	Lawson
TOTAL, .	226	36	15	277	

"It will be observed that the mortality and sickness has been greatest amongst the plants brought out under the care of the members of the Forest Department. I have no doubt this result is due to over-kindness during the voyage. The plants have been apparently freely watered and over-shaded; and in the close and moist atmosphere of the cases, unnatural forced growth has been the result. Mr Gamble's consignment is an exception, the plants brought out by him being in quite as good health as those that came untended in the 'City of Mecca.' The condition of the latter is wonderfully good, and indicates extreme care in the selection of plants, and in the mode of packing them.

"As soon as the plants shall have recovered a little from their journey, I propose to despatch them to Sikkim.

"I take this opportunity of stating that the twelve plants brought out in July last by Mr Walton were forwarded to Sikkim three months ago, and that eleven of them are now in excellent order; the twelfth unfortunately died during the journey to Sikkim.

"The condition of the eleven plants just alluded to, of the five old plants formerly sent from this garden to Sikkim, and of the young ones propagated from them, leads me to entertain hopes that in that province the Ipecacuanha experiment will be attended with great success."

A question has been started whether there are not plants in India which may be used as Ipecacuan. One of these is the *Tylophora asthmatica*, W. et A., an Asclepiadaceous plant, which

has been known under various names:—*Cynanchum Ipecacuanha*, Willd.; *Asclepias asthmatica*, Roxb. Fl. Ind.; *Cynanchum vomitorium*, Lam. Dr Roxburgh and Dr Anderson used the plant for dysentery in India with great success.

There are some peculiar features in the plant now under cultivation which require investigation, and I am not able to give a full paper on the whole subject until further cultivation. The plant which has been long in the garden has flowered regularly. Even the young cuttings have sent forth their flowers. The plant, on the other hand, sent from Rio Janeiro, although treated in the same way as the other, has not flowered.*

The former, although flowering freely, has not produced perfect fruit until the present year. The plants were carefully fertilised by the application of the pollen of one flower to the stigma of another. By this means we have secured a number of fruiting specimens, and I now exhibit fruiting plants with drawings of the fruit and sections.

The fruit is drupaceous, of a dark purple colour, shining and glossy on the outside. It is about the size of a large currant, and when ripe it falls off easily. Each fruit contains two seeds. These are seen in the section of the fruit. The albumen of the seed is very hard. I have not seen any figure of the fruit in botanical works containing plates of the plant. There is a resemblance between it and that of *Psychotria emetica*.

We expect that some of the seeds will ripen, and that we shall then be able to propagate the plant from seed.

The following Gentlemen were elected Fellows of the Society:—

George Seton, M.A. Oxon., Advocate.
Captain Charles Hunter.

* Since this communication was made the plant has flowered, and has shown peculiarities in the relative length of the stamen and pistil. July 1872.

Monday, 1st April 1872.

PROFESSOR SIR ROBERT CHRISTISON, BART., President, in the Chair.

The following Communications were read:—

1. On Cardiocarpon. By Professor Duns, D.D., F.R.S.E., New College.

The attention of the Society was called to many beautiful specimens of Sphenopteris laid on the table. These had been obtained by Dr Duns and his predecessor, Dr Fleming, from the old workings in the Burdiehouse limestones, near Edinburgh, well known from Hibbert's Memoir (1835), and from the papers of more recent observers. The species exhibited were chiefly *S. artemisiæfolia* and *S. affinis*. An Antholite (*A. Pitcairniæ*) was also shown, in which the pedicels that spring from the flower-like buds in the axils of the bracts, sub-opposite in the spike, are well represented. The author then referred to *Cardiocarpon*, Brong., and to the species named by Brongniart, Lindley, and Hutton, and more recently by Dawson and Lesquereux. It was pointed out, that very many *Cardiocarpa* occur in association with the specimens of Sphenopteris on the table. On three of these alone there are above 160. Of these, some are almost globular, others are oval. Some taper to a single sharp point; others, and the majority, have an acute bifid apex. In many the medial ridge is not seen, in others it is highly marked. In a few this ridge has an excurrent appearance, both at the apex and at the supposed point of attachment to the plant. Many of the forms are so placed as to present an appearance of organic connection with the Sphenopterides. The author then showed that it "is needful to guard against a tendency to give undue importance to the mere fact of association. If in other departments this has lead to most erroneous inferences, it will be sure to mislead in the study of palæobotany. Some weight is, no doubt, to be given to the fact, but to use it to any extent as a guide in determining the affinities of fossil plants is, to say the least, not safe. Principal Dawson has pointed to the occurrence

of *Cardiocarpa* along with the stems of *Sigillaria* as corroborative of the theory of the conifer or cycad character of *Sigillaria*. He says, "Some botanists, conspicuous among whom is Brongniart, hold that *Sigillaria* were gymnospermous plants allied to Cycadaceæ. Others are disposed to regard them as Acrogens, and as closely allied to Lycopodiaceæ. . . . . . In favour of the former view we may adduce the exogenous structure of the stem of *Sigillaria*, and the obvious affinity of its tissues to these of conifers and cycads, as well as the constant association with trees of this genus of the evidently phanerogamous fruits, known as *Trigonocarpum* and *Cardiocarpum*." And he adds, "The higher *Sigillariæ* unquestionably resemble cycads in the structure of their stems. Their long, rigid, narrow leaves may be compared to single pinnæ of the leaves of cycads. Their cord-like rootlets, as I have ascertained by actual comparison, are similar to those of cycads. If their fruit was of the nature of *Cardiocarpon* or *Trigonocarpum*, this would also correspond." (See *Quarterly Journal of the Geological Society, May* 1871.) This assumes throughout that palæobotanists are agreed as to the nature of these fossil fruits, which is far from being the case.

In August 1870, Mr C. W. Peach, to whom Scottish natural science is so much indebted, found specimens of *Cardiocarpon* organically united with a plant long known by the name, *Antholites Pitcairniæ*. The specimens were obtained from carboniferous shale at Cleuch, near Falkirk. Specimen No. 16, on the table, is *Antholites Pitcairniæ*, from shale near Bathgate. By the kindness of Mr Peach, I am able to show the Society an example of *Antholites* with the fruit organically attached. The importance of this discovery is at once recognised. In a department where facts are the letters, and their association the words by which we read the history of creative manifestation, every worker will acknowledge the value of an observation like that referred to, even though he may not see his way to accept views implying generic identity between the fruit now associated with *Antholites* and *Cardiocarpon*. On the assumption of this identity, Mr Carruthers has recently limited the term *Antholites* to the place, or rather the use assigned to it by Brongniart—"Les espèces indéterminable sont généralement désignées sous le nom d'Antholites."—*Prod.* p. 149. In-

stead of *Antholites Pitcairniæ*, Lindley, he has proposed *Cardiocarpon Lindleyi*, Carruthers. (*Geolog. Mag.*, *Feb.* 1872., pp. 54–57.) Along with a figure of the Falkirk specimen, another is given from an unknown locality, supposed to be from mines in Derbyshire. The fruit on the latter is regarded as similar to *Cardiocarpon acutum* of Lindley.

It was stated that, so far as the author is aware, there is no certain record as to the form of the fructification of such Sphenopterides as *S. artemisiæfolia* and *S. affinis*, or, indeed, of any of the species closely related to these by their bipinnate leaf and the deep pinnatifid segments of their leaflet. Gœppert and Unger's statement, that the fructification is "punctiform or marginal," may be true of species like *S. dilata*, or *S. latior* (Dawson), but these differ widely from the specimens now noticed, though they bear some resemblance to living forms. As regards *S. artemisiæfolia*, Brongniart himself has said, that he has not been able to find the least resemblance between it and living ferns. It was shown that this remark is especially applicable to *S. affinis.* The question seemed to be raised by what might be said to be the almost constant association of *Cardiocarpa* with these two species, "Have they their proper place under the genus *Sphenopteris?*" Dr Duns stated in conclusion, that while these species must still be regarded as true ferns, and while the idea even of organic connection between such forms as the samaroid fruit *Cardiocarpon* and the species *S. artemisiæfolia*, and *S. affinis* is opposed to all accepted views of plant affinity, yet the association, as shown in the numerous specimens on the table, is so frequent, and often so remarkably like organic, as to call for the attention of observers.

2. On the Composition of the Flesh of the Salmon in the "Clean" and "Foul" condition. By Sir Robert Christison, Bart.

Having had occasion lately to fill up some blanks in a table of the Nutritive Value of different kinds of Food, I was unable to find for the purpose an analysis of the flesh of the Salmon. I have therefore made such an analysis as is necessary; and as

the results may be useful to others, I beg to offer them to the Society.

I first examined the composition of a very fine "Clean" fish, caught in the estuary of the Tay in May last year, and weighing 20 pounds. I have never seen a finer fish from that far-famed salmon-river.

I have also, in contrast with this, examined a "Foul" fish, or Kelt, taken in the beginning of March last from a pool where spawned fish are known to congregate at that season in the Isla, a principal tributary of the Tay. It weighed 27 pounds the day after it was caught, and would probably have weighed 35 pounds in good condition. In order to account for my being in lawful possession of such an article, I must mention that I owe it to the consent of the Commissioners for the Tay Fisheries, whose kindness in presenting, for a scientific object, what otherwise cannot be easily obtained without infringing the law, may receive, as I hope, some return in the additional proof which analysis supplies of the inferiority of the salmon as food when in the state of a Kelt, and the folly of destroying it before it recovers condition.

The clean salmon of last May presented abundance of fat under the skin, and in masses betwixt the muscles. Avoiding all accumulations of fat in mass, I cut one piece of muscle from the dorsal region a little in front of the dorsal fin, and another from the ventral region directly opposite; so that the one should represent the "thick," and the other the "thin," of a slice of salmon Four hundred grains of each being cut into fine chips about twelve hours after the fish was caught, each was separately exhausted by ether; and the ether was distilled off at a gentle heat. When the residual oil was deprived of a little adhering alcohol and water by heating it gently for an hour in an open vessel, it had a bright amber colour, and a strong odour not very different from that of cod-liver oil. The fibrous residuum was dried at 212° till it ceased to lose weight. A portion of the dry residue was incinerated in order to determine the fixed saline constituents. The difference denoted the dry nitrogenous nutritive principles, fibrin, albumen, and extractive matter usually called osmazone.

The results were as follows:—

	Dorsal.	Abdominal.	Mean.
Oil	16·66	20·4	18·53
Fibre, albumen, extractive matter .	20·57	18·82	19·70
Saline matter .	0·88	0·88	0·88
Water . . .	61·89	59·90	60·89
	100·00	100·00	100·00

The Kelt of last March was as ugly a specimen of the *Salmo Salar* as I have ever seen. It was 38 inches long, weighed 27 pounds, and was very lank in the belly, soft in the flesh, much lacerated in the dorsal fin and tail, and of a uniform, disagreeable, mottled-grey colour over the entire skin. In its structure otherwise it was a true male salmon. I subjected it to analysis in the same way as the clean fish, with the following results. The analysis was made about forty-eight hours after the fish was caught; and in the interval it was shut up in a box, so that there could not have occurred any appreciable loss by evaporation.

	Dorsal.	Abdominal.	Mean.
Oil	1·2	1·30	1·25
Fibrin, albumen, extractive matter .	16·92	17·22	17·07
Saline matter [inferred from the former analysis] . . .	0·88	0·88	8·88
Water	81·0	80·60	80·80
	100·00	100·00	100·00

Thus it appears — 1. That the nitrogenous solids of a Clean salmon, and its oil or fat, constitute together in round numbers 38 per cent of its flesh; the remaining 62 per cent being water, with a little saline matter (0·9 per cent.). 2. That the fat and the nitrogenous constituents are nearly equal to one another. 3. That there is decidedly more fat in the "thin" or abdominal region than in

the "thick" or dorsal region, but somewhat less of nitrogenous constituents. 4. That there is very little difference in constitution between the dorsal and abdominal regions of a "Foul" fish or Kelt. But, 5. That the Kelt is a much more watery fish than the clean salmon; and that this is slightly owing to a deficiency in nitrogenous ingredients, but much more to an enormous deficiency of oil or fat,—which is reduced to almost a sixteenth only of its amount in a clean-run fish.

I am not aware of any good authority for the prevalent notion that a Kelt is unwholesome food. But it is plain from the foregoing analysis, that the Parisian gastronome,—who, before the late stringent measures against river-poaching in Scotland during close-time, consumed a large proportion of Scottish Kelts,—must have been indebted for his enjoyment therein much more to his cook than to his fish. On the other hand, it is easy to see why an Apicius, whose taste has been cultivated on the banks of a Scottish salmon-river, should wonder how any one can imagine, that the delicate flavour of a fish in good condition is improved by besmearing it with butyraceous sauces, simple or compound.

3. On Recent Estimates of Solar Temperature. By James Dewar, Esq.

(*Abstract.*)

After referring to the recent discussion on the temperature of the sun, in which Secchi, Zollner, Vicare, Deville, and Ericsson have taken part, the author proceeds to group all the known methods of arriving at a knowledge of high temperatures under eight different processes. The following table gives the names of the physicists who have specially employed each process, together with the principle on which it is founded:—

(1.) Guyton and Daniell, Prinsep, &c.—Expansion of Solids and Gases.

(2.) Draper.—Refrangibility of Light.

(3.) Clement and Desormes, Deville.—Specific Heat.

(4.) Becquerel, Seamens.—Thermo-Electricity and Electric Conductivity.

(5.) Bunsen, Zollner.—Explosive Power of Gases.

(6.) Newton, Waterston, Ericsson, Secchi.—Radiation.

(7.) Thomson, Helmholtz.—Mechanical Equivalent of Heat.

(8.) Deville, Debray.—Dissociation.

After treating of the great disparity of opinion regarding the temperature of the sun, the author proceeds to detail how it is possible, from the known luminous intensity of the sun, to derive a new estimate of solar temperature. This calculation is based on a definite law relating temperature and luminosity in the case of solids, viz., the total luminous intensity is a parabolic function of the temperature, above that temperature where all kinds of luminous rays occur. So that if T is a certain initial temperature, and I its luminous intensity, a a certain increment of temperature, then we have the following relation:—

$$T + n(a) = n^2 I.$$

The temperature T is so high as to include all kinds of luminous rays, viz., 990° C., and the increment a is 46° C. This formula expresses well the results of Draper, and I have used his numbers as a first approximation. It results from the above equation, that at a temperature of 2400° C., the total luminous intensity will be 900 times that which it was at 1037° C. Now, the temperature of the oxyhydrogen flame does not exceed 2400° C, and we know from Fiseau and Foucalt's experiments that sunlight has 150 times the luminous intensity of the lime light; so that we only require to calculate at what temperature this intensity is reached in order to get the solar temperature. This temperature is 16,000° C., in round numbers. Enormously high temperatures are not required, therefore, to produce great luminous intensities, and the temperature of the sun need not, at least, exceed the above number. Sir William Thomson, in his celebrated article, "On the Age of the Sun's Heat," says, "It is almost certain that the sun's mean temperature is even now as high as 14,000° C.," and this is the estimate with which the luminous intensity calculation agrees well.

4. On the Temperature of the Electric Spark. By James Dewar, Esq.

(*Abstract.*)

The author begins this paper by calculating the highest hypothetical temperature that could be produced by the chemical combination of the most energetic elements if all the heat evolved could be thrown into the product. This would not exceed 19,500° C. in the case of silica, and 15,000° C. in the oxides of aluminum and magnesium, and these are the highest results. The estimation of the temperature of the electric spark is based on the thermal value of each spark, together with the volume of the same. The methods of observing these quantities are fully detailed in the memoir. The general result may be stated thus,—the temperature of the electric spark used in the experiments ranged between 10,000° C. and 15,000° C.

The following Gentlemen were admitted Fellows of the Society:—

James Thomson Bottomley.
Thomas Knox, Esq.
Dr D. Argyll Robertson.

Monday, 15th April 1872.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Action of Water on Lead. By Sir Robert Christison, Bart.

After summarising the conclusions at which he had arrived from numerous experiments made more than forty years ago, as published in his Treatise on Poisons, and in the Transactions of this Society, the author alluded to various blanks left at that time in the inquiry which had not been yet filled up, and to various criticisms and doubts which had been recently expressed relative to the facts and principles formerly announced.

The general results of the former inquiries are—1. That the purest waters act the most powerfully on lead, corroding it, and forming a carbonate of peculiar and uniform composition; 2. That all salts impede this action, and many prevent it altogether, some of them in extremely minute proportion; and 3. That the proportion of each salt required to prevent action is nearly in the inverse ratio of the insolubility of the compound which its acid forms with the oxide of lead. The effect of certain inorganic and organic ingredients of water in modifying the preservative power of the salts was not investigated, but has been since made the subject of numerous observations and inquiries by others, chiefly, however, of a desultory nature, some of them much too succinctly described, and some also of questionable accuracy.

The first part of the present paper dealt with the influence of inorganic substances. The second part, on the influence of organic matters, was reserved for a subsequent article.

It had been denied thatwater acts by reason, and in the ratio, of its purity; and it had even been alleged that distilled water itself does not act, if really quite pure. The author, however, had invariably found the reverse, and could assign no other explanation of these statements except some error in manipulation. For example, a very pure spring water was sent to him from the south of England, with the assurance that it had been found to be incapable of attaching lead. But, on making trial of it, he had found it act with an energy not inferior to that of distilled water. Also, it had been stated that ordinary distilled water is apt to contain a trace of nitric or nitrous acid, from nitrates incidentally present in the water subjected to distillation; and that such water, if distilled after the addition of a little potash to fix the acid thoroughly, yields a distillate which has no action on lead. But when the author prepared distilled water in this way, with great care to prevent the access of impurities from other sources, the only result was that the action was even greater than that of the ordinary distilled water of the laboratory, and so great as he had never observed before.

An interesting statement had been made by Dr Nevins, that some salts appear to allow of a certain action going on when they are present largely in water, although their influence, when they

exist in very small proportion, is to act as preventives. The author sometimes obtained the same result, and found the action such as might prove dangerous. But its limit requires to be defined; and there is reason to suppose that the proportion required to permit action will be found so great as never occurs in the instance of waters applicable to household use.

It has been also stated, but in general terms, without experimental proof, that the presence of carbonate of soda, even in a hard water, takes away the preventive influence of other salts, and enables water to dissolve lead. There appears to be some foundation for this statement. But here, too, it is necessary to fix what is the limit to such influence before its importance can be valued. Moreover, as bicarbonate of soda appeared to the author to have no such effect, and this is the usual form of the carbonate in natural waters, the practical importance of the fact is inconsiderable.

The author called attention to some observers not having understood the nature of the corrosive action of water on lead, and having confounded it with other causes of corrosion. Thus the true action has been confounded with the corrosive action of potent agents accidentally coming in contact with the metal in presence of water, —as, for example, when a lead pipe has been led through fresh mortar which is frequently or permanently kept moist, or when lumps of fresh mortar have been allowed to fall upon the bottom of a lead cistern. Several remarkable examples of rapid corrosion of this local kind were exhibited. The true or simple action of water had been not infrequently confounded also with the effects of galvanic action. Thus, if a lead pipe or cistern be soldered with pewter-solder, and not with lead, erosion takes place near the line of junction of the solder with the lead, of which characteristic examples were shown. The presence of bars of other metals crossing lead, or bits of them lying on it, will also develope the same action; and some facts seem to point to the same property being possessed in a minor degree by some stony and earthy substances. This observation may explain the local erosion sometime observed in cisterns containing hard water; since, if galvanic action be excited, it will be increased by saline matter existing more largely in these waters than in soft, or comparatively pure, water.

Lastly, some observers have contradicted former statements,

because in certain circumstances, which led them to anticipate no action, they nevertheless found lead in water, but only in extremely minute and unimportant proportion. The test for lead, the hydrosulphuric acid, when employed in the way now usually practised, is so delicate as to detect that metal dissolved in ten million parts of water, and even more. But facts warrant the conclusion that the impregnation must amount to at least ten times as much before water can act injuriously on man, however long it may be used.

2. On the Preservation of Iron Ships. By James Young, Esq., of Kellie.

My attention was called in January last year to the rusting of iron vessels by observing that the bilge water of my yacht (the "Myanza," 214 tons) was much discoloured by red oxide. Knowing that bilge water is apt to become acid, and thus to attack iron, the result was easily accounted for. Even when the water does not become acid, we may expect some action on the iron to take place when sulphuretted hydrogen exists, as it frequently does there, in which case, first a sulphide, then an oxide, and some sulphate, are formed. The remedy seemed to be easy, because the acid can be neutralised by lime. This earth would also prevent the formation of sulphuretted hydrogen.

I put this immediately into practice, adding lime until the bilge water was alkaline; and samples were taken every fourteen days, which showed the amount of rust to be rapidly diminishing. After six months the liquid became perfectly clear, so that the cure is complete. The yacht is a composite one, and the action is therefore greater than in iron vessels generally, because of the copper or cupreous bolts which are used. These bolts cause galvanic currents with the iron, and greatly assist in its oxidation and solution.

As a very little lime will last a long period, the plan causes neither trouble nor expense. Seeing in the newspapers that the destruction of the "Mægara" was attributed to the action of bilge water, I thought that my experience might be of some value.

3. First Report by the Committee on Boulders appointed by the Society.

In April 1871, a paper was read in this Society proposing a scheme for the conservation of boulder or erratic blocks in Scotland, in so far as they were remarkable for size or other features of interest. The Council of the Society approved of the scheme, appointed a committee to carry it out, and agreed to aid in meeting the expense of any circulars which might be necessary for conducting the inquiries.

The objects of the committee were twofold. They were first to ascertain the districts in Scotland where any remarkable boulders were situated; and, second, to select those which might be deemed worthy of preservation, with the view of requesting landed proprietors and tenants of farms not to destroy them.

The labours of the committee have as yet been directed only to the first of these objects.

In order to procure information, they drew out a set of printed queries, applicable to boulders apparently above 50 tons in weight, in order to ascertain the parishes in which they were situated, and the names of the proprietor and tenant on whose lands they were; and also to learn other features, such as the nature of the rocks composing the boulders, their form, and the existence of striations upon them. Inquiry was also made whether the boulders had any traditional names or popular legend connected with them, or exhibited any artificial markings.

The committee thought that, with a view to the conservation of the boulders, the greater the interest which could be shown to attach to them, the more chance there would be of inducing proprietors and tenants to preserve such as the committee might select for preservation.

Besides queries about boulders, there was one query directed to ascertain the occurrence of *kaimes* or *eskars*, *i.e.*, long banks of sand and gravel, as some persons imagined that the agents which transported boulders might have had some relation with, or might throw some light on those which were concerned in the formation of those deposits.

Circulars containing queries, a copy of the minute of Council

approving of the scheme, and appointing a committee, and an abstract of the paper read in the Society in April 1871, explaining the scheme, were transmitted to the ministers of all rural parishes in Scotland.

About 700 circulars were issued. After the lapse of six months about 100 answers were received.

The committee, on considering these, were of opinion, that in making their queries applicable only to boulders exceeding 50 tons in weight, they had probably erred, by excluding many boulders of interest, and to this circumstance they attributed the small number of answers sent.

They therefore resolved to issue another circular containing the same queries as before, to cover boulders exceeding 20 tons in weight. This circular was addressed to parochial schoolmasters, as the committee feared that they might be considered troublesome, if they made a second application to ministers of parishes.

This second circular brought to the committee a large amount of information, and they desire now to express their cordial thanks to both classes of reporters for responding so readily.

When the committee was appointed, an expectation was expressed that they should, from time to time, lay before the Society some account of their proceedings, and of the progress made by them.

In now proceeding to the performance of this duty, the committee will confine themselves to a statement of facts communicated, and avoid at present attempting to draw conclusions from these facts.

1. In order to show the situations of the boulders reported on, the committee have drawn up a list,* according to counties, giving the names of the parishes where boulders occur, adding shortly any particulars regarding them, such as size, nature of the rock composing the boulder, direction of the longer axis, striations, popular names, and legend, if any.

They have also, on a general map of Scotland, indicated by a red cross the exact position of the most remarkable boulders.

From this table and map, it will be seen that *Aberdeenshire* possesses the largest number of boulders, and also the boulders of greatest magnitude.

* This list is in the Appendix.

Ross and Cromarty stand next, then *Perth*, *Argyll*, *Inverness*, *Kirkcudbright*, and *Forfar*.

2. In regard to *size*, the largest boulder reported is one of granite, in the parish of Pitlochry, called "Clach Mhòr" (big stone), being about eight yards square, and estimated about 800 tons.

There are two boulders between 500 and 600 tons weight, one in Ross, the other in The Lewis.

There are three boulders between 200 and 500 tons, seven between 100 and 200 tons, twenty between 50 and 100 tons.

3. With regard to the *nature of the rocks* composing the boulders, the largest reported are of granite, though there is one known to the convener of the committee, still larger, of conglomerate, in Doune parish. The most numerous are composed of compact greenstone; but these are generally of small size. The next most numerous class are of grey granite. There are also many of gneiss, grey-wacke, and conglomerate.

4. The boulders reported generally *differ* in regard to the nature of the rocks composing them, from that of the rocks of the district in which they are situated; and, in many of the reports, reference is made to the district from which the boulder is supposed to have come.

Thus, in those parts of Perthshire, Forfarshire, and Kincardineshire where the old red sandstone formation prevails, and over which multitudes of granite, gneiss, and conglomerate boulders are lying, most of the reporters have no hesitation in pointing out that the parent rock is in the Grampian range, lying to the north or west. So also in Wigtonshire, where the greywacke formation prevails, and on which many boulders of grey granite are lying, the general opinion is that they came from the granite hills of Kirkcudbrightshire.

But where a boulder happens to be of a species of rock the same as that of the rocks of the neighbourhood, it is more difficult to recognise it as a true erratic. Hence, in the Lewis, where there are huge single blocks of gneiss, which is also the prevailing rock of the country, the reporters say that they cannot tell whether these blocks are erratics or not.

5. The boulders mentioned in the reports are of various *shapes*. Some approach a cube, well rounded of course on the corners and sides. That is the shape mostly possessed by granite boulders.

Others again are of an oblong shape, and this is particularly the case with whinstone and greywacke boulders. The difference in this respect is probably mainly due to a difference in the natural structure of the parent rocks.

A point of some importance occurs in regard to oblong-shaped boulders.

The direction of their *longer axis*, in the great majority of cases, is stated to coincide with the direction in which they have come from the parent rock, when the situation of that rock has been ascertained. Thus, in Auchterarder parish, there is a boulder 10 feet long by 6 broad, the longer axis of which points north-west. In Auchtergaven parish there is a granite boulder 10 feet long by 8 broad, the longer axis of which points due north. In Menmuir parish, Forfarshire, there are two large granite boulders, the one 14 by 9, and the other 13 by 9, the longer axis of which points north-west. In each of these cases the reporters seem satisfied of the situation of the parent rock, and in each case the longer axis of the boulder points towards it.

It appears, also, that where there are natural *striations or ruts* on the boulders, these almost always run in a direction parallel with the longer axis; and that when there are striæ crossing these, the number of such oblique striæ are comparatively few.

6. Notice in the reports is taken of the remarkable *positions* occupied by some boulders.

Thus, the Ardentinny report refers to a large boulder called "*Clachan Udalain*," or the nicely balanced stone,* so-called, as the reporter states, because "it stands on the very edge of a precipice, and must have been gently deposited there." In the same parish there is another boulder, called "*The Giant's Putting Stone*. It is pear shaped, and rests on its small end. It looks," says the reporter, "as if a push would roll it over."

In Menmuir parish (Forfarshire), two boulders are reported, each from 30 to 40 tons in weight, and perched on or near the top of a hill, having come there, as the reporter thinks, from a parent rock 15 miles distant, with several valleys intervening.

Cases of the same kind are reported from islands.

On Iona, near the top of the highest hill in the island, which is

* Another translator represents this word to mean "*of the swivel.*"

about 250 feet above the sea, there is a great boulder of granite. There is no granite in the island. The nearest place where that rock occurs is in the Ross of Mull, with an arm of the sea intervening.

In the Island of Eday, in Orkney, there is a conglomerate boulder, called the "*Giant's Stone*," about 8 tons in weight, near the top of a hill—the only one in the island—about 300 feet high. There is no conglomerate rock in Eday. But conglomerate rock occurs in the Island of Stronsay, situated to the south-east, a few miles distant.

7. The report from the parish of Benholm (Forfarshire), by the Rev. Mr Myres, gives information and suggestions to the committee of considerable interest. On the sea coast of that parish, two sets of boulders are described. One set are supposed to have come from the Grampian range many miles to the north-west, and consist of granite and gneiss rocks. But another set, also consisting of primitive rocks, are believed to be derived from a different source altogether, viz., from the great beds of conglomerate rock, which forms a band crossing the whole of Scotland from Stonehaven and Bervie, in a south-west direction, to Dumbarton and Rothesay. Some of the rounded masses in the conglomerate are stated to be several feet in diameter, and a few present appearances of striation; a fact which, if established, would seem to prove that, at a very early period indeed, ice action had existed, and had formed boulders just as it did at a later period.

This report from Benholm parish was read lately at a meeting of the Geological Society of Edinburgh, and was illustrated by drawings and specimens which afforded strong evidence of the correctness of these views.

8. With regard to *kaims* or long embankments of gravel or sand, there are twenty-three parishes reported to the committee as containing them.

They appear to be most numerous in Aberdeenshire, Forfarshire, and in the east of Perthshire. In Kemnay parish there is a kaim said to be 2½ miles long, running east and west. In Airlie parish there is a kaim 2 miles long, also running east and west. In Fettercairn parish, Kincardineshire, and also in Tarbet parish, Ross-shire, there are several kaims parallel to, and not far distant from, one another.

In two cases the reporters, who seem to have visited Switzerland, whilst mentioning kaims in their parishes, express an opinion that they are evidently lateral and terminal moraines.

In several cases, oddly enough, these kaims exist at much the same level above the sea, viz., between 700 and 800 feet, which happens also to be the height of similar deposits in Berwickshire and Mid-Lothian.

The committee wish it to be understood, that in the present report, they confine themselves simply to a statement of the information received. They do not think it would be wise as yet to attempt to draw theoretical conclusions. Almost every day they are receiving more answers to their circulars; and they think that the wider the basis for considering the important geological questions connected with the transport of boulders and the formation of kaims, there will be the more probability of reaching the truth.

One object which the committee have in view in explaining the nature of the information communicated to them, is to show and to acknowledge the deep debt of gratitude which this society lies under to the gentlemen who have responded to the circulars of the committee.

But whilst the information supplied is undoubtedly valuable, the committee cannot but feel the truth of what many of the reporters themselves modestly and properly state, that they are so little acquainted with geology or mineralogy, that they may not have correctly understood the queries, or they may not have made their observations in the way necessary to answer the queries. Moreover, the committee itself may not in all cases have rightly understood the answers given.

Having regard to these considerations, the committee would very much desire that the boulders reported should be examined by experienced geologists, who should at the same time make a survey of the district, in order to see whether it presents any special features bearing on the nature of the agency by which the boulders were transported. The information in the reports received by the committee would greatly facilitate such an inspection, as they indicate not only the parish and the farm where

the boulder is situated, but generally record other features of interest.

The committee entertain a hope, that were this wish on their part made known, some geologists, who may be either resident in Scotland or who may purpose to visit Scotland during the course of the ensueing summer or autumn, might offer their services in the way, and for the purpose now suggested. In that case, the committee would willingly lend the reports which they have received, on condition that the results of the inspection were made known to the committee.

The committee will place in the library of this Society, the list of boulders before referred to, showing the parishes in each county in which the boulders and kaims are situated, so that any person may see where these parishes are, and be able to judge whether it would be convenient for him to visit these.

Were this list published, and generally circulated, good would result in another way. As it would show all the parishes from which reports of remarkable boulders and kaims had come, some persons might be able to discover parishes from which reports had been omitted to be sent, and if these were pointed out to the committee, they would make the requisite inquiry.

II. The committee proceed next to notice points of *archæological* interest connected with boulders.

1. The committee were surprised with the large number of individual boulders possessing names by which they were known in the district.

The names may be classified under several heads:—*First*, there are names having reference to the agency by which the boulders were supposed to have come into the district. *Second*, there are names indicative of the use to which boulders were put. *Third*, there are names making the boulders commemorative of certain events.

Many of the boulders, besides having a name, have also a *legend*, which explains and illustrates the name.

The *Giant's Stone*, *Fingal's Putting Stone*, the *Witches' Stone*, the *Carlin Stone*, *Heathens*, *Hell Stones*, the *Deil's Stone*, the *Deil's Putting Stone*, the *Deil's Mither's Stone*,—these are among the names, almost all in the Gaelic language, which ap-

parently were given to account for the way in which particular boulders came into the district. *

To show that this was the origin and object of the names, a few of the legends, as stated in the reports, may be given. They indicate, no doubt, a very deplorable state of ignorance and credulity; but they indicate also that in many cases our forefathers had satisfied themselves that the boulders had been transported into the district. Their perplexity was how to account for their transport. Not knowing anything of glaciers or icebergs, they had to resort to supernatural agency for an explanation. A few examples may be given.

Reference has already been made to a large conglomerate boulder near the top of a hill, in the Island of Eday, one of the Orkneys. It goes under the name of "*Giant's Stone.*" The legend for it is, that it was flung by a giant from the Island of Stronsay. Now, as already stated, there is no conglomerate rock which could have supplied the boulder in Eday Island, but there is in Stromsa.

So also in the Island of Sanday, one of the Orkneys, there is a granite or gneiss boulder; the legend about which is, that it was thrown from the Shetland Islands by a giantess, who had been jilted by a Westray man. She intended to throw it into Westray, but she made a bad shot, and it fell into the Island of Sanday. There is no rock which could have produced the boulder in Sanday, but there is abundance of it in the Shetlands.

About 1½ miles west of St Andrew's in Fife, there is a large conglomerate boulder, and the legend about it is, that when the "*Four knockit steeple*" in that town was being built, a giant who lived at Drumcarro Crags, a hill about 5 miles to the north-west of St Andrews, was indignant, and resolved to demolish the edifice. He, therefore, got the largest stone he could find, and borrowing his mother's apron, he made a sling of it, and threw it at St Andrews. But the stone being too heavy, the apron broke, and the stone did not quite reach its destination, and there it has lain ever since. There is no conglomerate rock where the boulder lies, but there is at or near Drumcarro Crags.

* The Rev. Mr Joass of Golspie refers to a boulder in Sutherland, called "*Clach Mhic Mhios,*" or stone of the Manthold son, believed to have been thrown from a hill two miles off by Baby Fingalian.

The Witches' Stone, which is on the estate of Pitferran, near Dunfermline, has this legend: A witch who lived among the hills to the west, wishing to confer a favour on the Pitferran family, resolved to give them a cheese-press, the heaviest she could find. She selected a large block of basalt of the proper shape, and carried it in her apron, which, however, broke under the load before she reached the family residence; and there it has lain ever since. There is no rock of that kind near Dunfermline, but there is to the westward.

In the parish of Carnwath there are one or two spots where there are or have been groups or collections of whinstone boulders, between the river Clyde and a hill of whinstone, known by the name of the *Yelpin Craigs*. The distance between the river and this hill is three or four miles. These heaps of boulders have from time immemorial gone by the name of *Hellstanes*, insomuch that places near them are called *Hellstanes Loan*, *Hellstanes Gate*, &c. The legend is, that Michael Scott and a great band of witches, wishing to dam back the Clyde, gathered stones at the *Yelpin Craigs*, and were bringing them towards the Clyde, when one of the young witches, groaning beneath her load, cried out, "Oh Lord, but I am tired." As soon as she uttered the sacred name, the spell broke, the stones fell down, and have remained there ever since.*

There are many legends founded on the agency of the devil, and on his hatred of churches and clergy. Thus near the old church of Invergowrie, now in ruins, there is a large whinstone boulder, called the *Paddock Stone*. The legend about it is, that the devil, going about in Fife, descried the church shortly after it was begun to be built, and wishing to stop the work, threw a large stone at it across the Frith of Tay. There is no whinstone rock at or near Invergowrie, but there is abundance of it immediately opposite in Fife.

In the parish of Kemnay (Aberdeenshire), there is a boulder of grey granite, called the *Devil's Stone*, estimated to weigh about 250 tons, which lies not far from the old kirk. There is no rock of that nature in Kemnay parish, but there is at Bennachie, a hill about seven or eight miles to the westward. The legend explain-

* This legend is given more fully in "Scenery of Scotland," p. 314, by Professor Geikie.

ing how this boulder came from Bennachie forms the subject of a ballad,* a few verses of which may be given.

" It was the feast o' Sanct Barnabas,
I' the merry month o' June,
When the woods are a' i' their green livery,
And the wild birds a' in tune;

" And the priest o' Kemnay has gaen to the kirk,
And prayed an earnest prayer,
That Satan might for aye be bund
To his dark and byrnand lair.

" And aye the haly organ rang,
And the sounds rose higher, higher,
Till they reached the Fiend on Bennachie,
And he bit his nails for ire.

" And he lookit east, and he lookit west,
And he lookit aboon, beneath;
But nocht could he see save the baul' grey rocks
That glower'd out through the heath.

" He lifted aloft a ponderous rock,
And hurl'd it through the air;
' 'Twere pity ye sud want reward
For sae devout a prayer!'

" The miller o' Kemnay cries to his knave,
' Lift up the back sluice, loon!
For a cloud comes o'er frae Bennachie
Eneuch the mill to droon.'

" The boatman hurries his boat ashore,
And fears he'll be o'er late;
Gif yon black cloud come doon in rain,
It 's fit to raise a spate.

" But the ponderous rock came on and on,
Well aimed for Kemnay Kirk;
And cross'd it field, or cross'd it flood,
Its shadow gar'd a' grow mirk.

" But the fervent prayers o' the haly priest,
And the power o' the Sanct Anne,
They turn'd the murderous rock aside,
And foil'd the foul Fiend's plan.

* From "Flights of Fancy and Lays of Bon Accord." By William Cadenhead Aberdeen. Edinburgh: Oliver and Boyd, 1853.

"And it lichted doon frae the darken'd lift,
Like the greedy Erne bird,—
And there it stands i' the auld kirk-lands,
Half-buried in the yird."

These legends, in explanation of the transport of Scotch boulders, are of the same nature as the legend which professes to explain how the Blue Stones of Stonehenge came to Salisbury Plain in England. Jeffrey of Monmouth, who was the first author to write a description of Stonehenge, says that certain of the stones were brought by Merlin and a band of giants from Ireland. Mr Fergusson, in his book on Ancient Stone Monuments, recently published, says that some geological friends of his have told him, that these blue stones of Stonehenge are a species of trap, which is not known in England, but is well known in Ireland; and therefore Mr Fergusson supposes that they probably were brought from Ireland in ships. It seems quite as likely that these blue stones were boulders, and were brought from Ireland by natural agency, and deposited on Salisbury Plain in that way. There are strong proofs to show that there was an agency of some kind which swept over Ireland from the westward, and brought boulders across what is now the Irish Channel to the south-west districts of England.

In these legends we see the efforts of the people in those early times to account, in the best way they could, for the transport of boulders into their districts. It is evident that they had investigated the subject, and had made considerable approaches to the truth. Finding that many of these great blocks differed in composition from all the rocks of the district where the blocks lay, and inferring that their rounded shapes were probably due to friction, they inferred that they must have come into the district from some distant quarter; and this inference was confirmed by discovering that in certain other districts there was rock of the same description as the blocks. But how blocks exceeding 100 tons weight could have been brought many miles, and over a tract of country uneven and broken in its surface, their knowledge of nature's laws did not enable them to explain. The only agency which they could think of was superhuman and supernatural; and hence the invention of such legends as assumed the agency of Merlin, giants, Michael Scott, witches, and the devil.

2. The second class of names by which particular boulders are known, have reference to the *uses* to which these stones were put.

In remote periods in the history of Scotland, when there were no maps, roads, or even names of parishes, it was important to have some other means of indicating spots or districts where people required to congregate for special purposes.

One of the boulders reported to the Committee (in the Island of Harris), still goes by the name of "*Clachan Treudach*," or the Gathering Stone.

What were the special purposes for which our early forefathers gathered together is of course not easily discovered. But the ancient names of the boulders seem to throw light on the subject. (1.) Such names as "*Clach-sleuchdaidh*," or Stones of Worship (in the parish of Kirkmichael); "*Clach an t-Tobairt*," or Stone of Sacrifice; "*Clach na Greine*," Stone of the Sun; "*Clach na h'Annait*." Stone of Victory, (a Scandinavian deity); and "*Clach mhòr a Che*," Great Stone of Che, (another deity), seem very plainly to indicate that these boulders were used as trysting-places for *worship;* and they were all the more suitable if they were looked upon with superstitious awe, on account of their supposed connection with spiritual agency. On two of the boulders reported to the Committee, there are artificial circular markings, other examples of which are very numerous throughout Scotland; and though archæologists are not yet agreed as to the meaning of these marks, one theory is, that they were symbols of a religious character. It is well known that these great stones were in some way or other, hindrances to the reception and diffusion of Christianity in most of the countries of Western Europe; for between the years 500 and 800 there are numbers of decrees and edicts requiring these stones to be destroyed, as being objects of superstition. There are some archæologists who go so far as to maintain that the word "Kirk" is actually synonymous with the word "Circle," meaning the circle of stones where Celtic worship was performed.

(2.) Another use to which these boulders were applied was *Sepulture*. There is in Berwickshire, a boulder known by the name of the "Pech or Pict's Stone," round which human bones in very large quantities were found a few years ago; and similar discoveries

have been made at boulders in many other districts, especially where they formed circles.

If these great boulders were used as places of worship, it was natural that they should also be used for sepulture, on account of the supposed sanctity of the place. Indeed, the fact of a place having been used for sepulture, creates of itself a presumption that it was used also for worship.

(3.) Another important purpose for which the boulders were used, was for the *trial of offenders* and the issuing of *judicial sentences*. Thus, in Little Dunkeld parish, there is a large boulder called "*Clach a mhoid*,"* or Stone of the place of Justice, where the baron of the district could try *offenders*, with right to hang or drown those convicted. In Ayrshire there is another large boulder called the *Stone of Judgment*, for the barony of Killochan. Several large rocking stones have been reported. In ancient times, when very rude tests of guilt or innocence were employed, the rocking stone was used in the trial of persons accused of crimes.

> "It moves obsequious to the gentlest touch,
> Of him whose breast is pure. But to the traitor,
> Though even a giant's prowess nerved him,
> It stands as fixed as Snowdon."

(4.) There are boulders which are known to have been used as *trysting places* for military gatherings; a large boulder on Culloden Moor is one example. It was on a whinstone boulder called *The Bore Stone*, that Robert Bruce planted his standard before the Battle of Bannockburn. A sandstone boulder on the Borough Muir, near Edinburgh, was the gathering point for the army collected by James IV. before the Battle of Flodden. Both of these stones are in existence. The Bannockburn stone is protected by an iron grating. The other stone is also preserved, being fixed on a wall near Morningside parish church, having on it a brass plate, bearing an inscription, given by the late Sir John Forbes.

(5.) Some boulders are said to have been used as trysting places for the *contracting* of *engagements*, such as matrimonial contracts, and others less important. There is a boulder in the parish of Coldstream (Berwickshire), called the *Grey Stone* from its colour, at which within the last hundred years marriages took place. The

* New Stat. Acc. vol. x. p. 1007.

bride and bridegroom stood on tiptoe on each side of the stone and joined hands over the top, whilst the friends of each party surrounded the stone to witness the engagement. The *Stone of Odin*, in the Orkneys, at which marriages were celebrated, was held in peculiar veneration; for in one case where a man was prosecuted for deserting his wife, it was stated to be an aggravation of his offence, that they had been married at the Stone of Odin.

3. A third class of names given to boulders had relation to them as *commemorative of important events.*

Thus there is in Badenoch the "*Clach an Charra*," or Stone of Vengeance, so called because a profligate and tyrannical feudal baron was killed by his own people near it.*

There is in Lewis the "*Clach D'hois*," or Stone of D'hois, a boulder of gneiss, weighing about 120 tons. It is called after a person named D'hois, who slew a giant near the boulder, and who also himself died immediately after, from the wounds received in the conflict.†

4. Some boulders were used to mark the boundaries of estates, parishes, and counties, and are still in many parts of Scotland recognised as affording evidence on that subject.

In Ross-shire, the boundary between the districts of Urray and Contin is marked by the boulder called "*Clachloundron.*"

A great boulder is said to indicate the spot where the three counties of Dumfries, Ayr, and Lanark meet.

The line of boundary between England and Scotland was in the eastern borders originally indicated by boulders, several of which still remain.

5. Some of the boulders have curious popular predictions connected with them.

Thus, near Invergowrie, on the north side of the Frith of Tay, there were in the days of Thomas the Rhymer two boulders entirely surrounded by the water, of which the seer sang—

"When Gows of Gowrie come to land
The day of judgment's near at hand."

These two boulders, called the Gows (probably because always frequented by sea-gulls), are now no longer surrounded by water.

* Proceedings Soc. of Scotch Antiquaries, vol. vi. 328.

† This Boulder and its legend reported to the committee by Captain Thomas, R.N.

But it is not they which have come to land, the land has come to them.

In the parish of Crieff a boulder of whinstone is reported, with a vein of white quartz through and partially round it, in consequence of which the stone has from time immemorial been known as the *Belted Stane.* The prediction about it is, that the white belt will gradually increase in length till it envelopes the stone; and that whenever the two ends meet, a great battle will be fought, on which occasion a king will be seen mounting his horse at the stone,—

> " 'Twixt the Gartmore Gap and *the Belted Stane*
> The nobles bluid shall run like a stream."

Geologists, however, are of opinion that there is not much chance of the quartz vein extending.

Perhaps some persons may think that the time of the Royal Society should not be taken up by any allusion to these absurd popular legends. There are, however, good reasons for referring to them. In the *first* place, they are evidence of the extraordinary ignorance and superstition which prevailed in former times in our own land, and even at no very distant date. In the *second* place, the archæological and even historical associations with which many of the boulders are invested, may induce many proprietors to take an interest in them and save them from destruction, if the committee think them worthy of preservation.

There is even yet among our fellow-countrymen a considerable amount of interest felt in these boulders, and particularly such as have traditionary names and legends; and it is to this feeling that several are indebted for their preservation. Professor Geikie at the last meeting of the British Association told this anecdote of the Ayrshire boulder, known as the Killochan Stone of Judgment. An enterprizing tenant, a stranger to the district, finding this stone much in his way, was preparing to blow it up with gunpowder. His intention becoming known, some of the old residenters went to the laird's factor and asked whether he knew what was intended. On his stating that he did not, he was entreated to prevent the stone from being destroyed. The proprietor was communicated with, and the new tenant was interdicted from meddling

with the stone. Shortly afterwards this inscription was put on the stone,—"*The Baron's Stone of Killochan.*" *

It is a boulder of blue whinstone, on which stands the market cross of Inverness. For some reason or other, it is preserved as the Palladium of the town, ever since the battle of Harlaw in the year 1411. It is called "*Clach na cudden,*" or "Stone of the tubs," from the circumstance that the people carrying water from the river used long ago to rest their tubs on it. It was till lately in the middle of the street; but having ceased to be of use, when water was brought into the town by pipes, it was removed to the side of the street opposite to the town hall, with the old cross of the town and the Scottish arms resting on it. "*Clach na cudden boys,*" is a *nom de guerre* for Invernessians; and "*All our friends round clach na cudden,*" is a toast given in many a distant land.

In the parish of Rattray, there is a remarkable boulder of micaceous schist, weighing about 25 tons, of which some account was given a short time ago in this Society. It bears a number of artificial markings of a very ancient date. The tenant of the farm on which it is situated proposed to blow it up. Some of the inhabitants having heard of this, went to the minister of the parish, and begged him to take steps to save the old stone of Glenballoch. The proprietor being on the Continent, the rev. gentleman applied to the factor, and through his good offices saved the stone. This gentleman being still under anxiety about it, lately requested this committee to communicate with the proprietor, Colonel Clark Rattray, with the view of obtaining from him a promise that the stone should be preserved. Colonel Clark Rattray was accordingly written to by the convener of the committee, and he at once acceeded to the request.

There is on the shore at Prestonpans, on the south side of the Firth of Forth, a large basaltic boulder, which has long been known under the name of "*Johnny Moat.*" The Convener wishing to see this boulder, he went out from Edinburgh a few weeks ago by rail to Tranent Station, and walked towards the shore in search of it. Between the railway station and Prestonpans he met a boy, whom he stopped, and telling him that he had come to see

* An account of this boulder was published in Macmillan's Magazine for March 1868, by Professor Geikie.

the boulder called "Johnny Moat," he asked the way. The boy pointed it out at once. Three or four other persons in succession, two of them women, had to be asked the same question before the spot was reached. Every one knew "*Johnny Moat.*" The last person accosted was a fisherman, and he volunteered to be guide. He seemed somewhat suspicious of the stranger's intentions; for after reaching the stone, he remained beside him till he saw it was only to measure its dimensions and make a sketch of it, that he had come. From what was observed during this visit, it was evident that every inhabitant of Prestonpans, not only knew of the boulder, but took a personal interest in it, and would sternly resist any attempt to destroy it.

It is satisfactory to find this popular feeling still prevailing to some extent. But the feeling is not of itself sufficient to prevent the wholesale destruction which is going on in many parts of Scotland. Thus, the minister of Bendochy reports to the committee, that "on the rising ground behind his manse, there was a circle of large stones, boulders, standing on their ends (Druidical); but some years ago they were removed. The place is yet called '*The Nine Stanes.*'"

There was formerly a rocking stone in Aberdeenshire, estimated at about 50 tons weight; but it has now been converted into field dykes.

Numberless cases of the same kind can be specified.

It is therefore most necessary to take steps to preserve what remain of these megalithic relics; and it is especially gratifying to the committee to be able to state, that the movement towards this object, made by this Society, has met with general approval.

The British Association, at its last meeting, so highly approved of the scheme, that it appointed a committee of some of its most influential geologists to carry out a similar scheme for England and Ireland.

In the last number of the "Geological Magazine," there is a laudatory notice of the object and operations of the committee; and the readiness with which all parties applied to in Scotland have responded to the circulars of the Committee, proves how much they also approve, to say nothing of express commendations contained in individual reports. Even in Switzerland notice has been taken

of our Scottish movement, and in very complimentary terms; for a few weeks ago, a pamphlet by Professor Favre of Geneva was received by the convener, alluding to our Society's movement in this matter, and anticipating important results from it.

List of Boulders reported to Royal Society, arranged by Counties and Parishes.

ABERDEEN.

Aberdeen (Town).—In excavating for foundation of house in Union Street, boulder of black sienite, 6 × 5 × 4 feet found. No rock like it *in situ* nearer than Huntly or Ballater, about 30 miles to N.W. or W. Under surface of boulder, striated. The direction of striæ coincides with the longer axis of boulder, viz., about east and west. Preserved, and set up in Court of Marischall College. (Reporter—Professor Nicol.)

Ballater.—On top of Morven, 3000 feet above sea, several granite boulders, unlike rock of hill, and apparently from mountains to west. (Jamieson, "Geol. Soc. Jour.," xxi. p. 165.)

Belhelvie.—Gneiss boulder, about 8 feet diameter, called the "Caple Stone," near parochial school. Rocks *in situ*; near it are granite. (Reporter—Alex. Cruickshanks, Aberdeen.)

Sienite boulder, in a wall, *King Street Road*, about $3\frac{1}{2} \times 2$ feet. The face covered with striæ parallel to longer axis.

Cairney Granite Quarry, 3 miles N.W. of Aberdeen, and about 400 feet above sea. When boulder clay removed, surface of rock found to be smoothed and grooved in a direction E.N.E. and W.S.W. (true.) (Reporter—Alex. Cruickshanks, Aberdeen.)

Bourtie.—1. Four Greenstone boulders, supposed to be Druidical; what is called "The Altar Stone," 16 × 6 × 5 feet, weighs about 18 tons. 2. Boulder, about 20 tons. Longer axis E. and W. Called "Bell Stane," the church bell having once hung from a post erected in it. 3. Whinstone boulder, about 20 tons, on Barra Hill, called "Wallace's Putting Stane," 24 feet in circumference. Legend, that thrown from Bennachie Hill, distant about nine miles to west. 4. Whinstone boulder, called "Piper's Stone." Origin of name given. 5. Whinstone boulder, called "Maiden Stane." Tradition

accounting for name. 6. Several Druidical circles described. (Reporters—Rev. Dr Bisset, and Mr Jamieson of Ellon.)

Braemar.—At head of Glen Sluggan, several large erratics. These stand exactly on watershed or summit level. Near shooting-lodge there, a cluster of four or five immense angular granite boulders. They touch one another, and may be fragments of one enormous mass. The adjacent rock is quartz. These blocks situated at end of a long low ridge or mound, which extends from south extremity of Ben Avon Hills, and which strewn thickly over with great granite blocks. The mound composed of a mixed debris of earth and stones, and is apparently a moraine. The adjoining mountain of "Cairn a Drochid" is composed of quartz and granite. On top of it are large granite boulders, many of which situated on quartz rock. (Reporter—Mr Jamieson, Ellon, in letter to convener.)

Chapel Garioch.—Boulder, $19 \times 15\frac{1}{2} \times 11\frac{1}{2}$ feet, weighing about 250 tons above ground. Height above sea 280 feet. Rests on drift. Longer axis E. and W. Legend, that thrown from Bennachie Hill to north-west. The rock of boulder differs from rocks adjoining. Kaims abound in parish. (Reporter—Rev. G. W. Sprott.)

Cruden.—In Boddom Dean, a granite boulder called "The Hanging Stone," measuring 37 feet in circumference and 27 feet over it, resting on several small blocks of granite. Supposed to be Druidical. Half a mile east there is another of 20 tons. (Buchan's Peterhead, published in 1819, and James Mitchell, Boddam.) Huge granite boulder, called "The Gray Stone of Ardendraught," broken up in 1777 to build walls of Parish Church. It was the stone on which "Hallow" fires* used to be lighted. (Jamieson, "Geol. Soc. Jour.," xiv. p. 525.)

* "Hallow" fires were lighted on 31st October, and were called "Saimh-theine." The "Beil-theine" fires were lighted on 1st May. These practices, formerly general in the Highlands of Scotland, were probably connected with the worship of the sun, whose departure in autumn, and return in spring, were signified by these rites. The Rev. Mr Pratt published an account of Buchan in the year 1858, and states (page 21), "Hallow fires are still kindled on the eve of All Saints, by the inhabitants of Buchan—from sixty to eighty fires being frequently seen from one point." (*Old Stat. Acct. of Scotland*, vol. xi. p. 621, and vol. xii. p. 458.)

At Menie Coast Guard Station, granite boulder, 54 feet in circumference and 7 feet above ground; also a greenstone boulder, 78 feet in circumference and 6 feet above ground. (Jamieson, "Geol. Soc. Jour.," xiv. p. 513.)

Near the "Bullers of Buchan," there stands "The Hare or Cleft Stone," which marks the boundary between the parishes of Cruden and Peterhead. Granite 9 × 8 feet, 160 feet above sea. (Pratt's "Buchan," 1858, page 47, and James Mitchell, Boddam.)

In this parish, and to north, numerous mounds and ridges of gravel, called at one place "Hills of Fife," at another, "Kippet Hills." The generic name of these mounds and ridges in this part of Scotland, is Celtic word "Druim" or "Drum." They are composed sometimes of sand, more frequently of gravel. The gravel consists of fragments of rock, generally from westward. They are always well rounded, by the friction they have undergone. They sometimes reach a size of 2 feet in diameter. The pebbles are chiefly gneiss.

On top of some of the knolls and ridges there are large boulders. There is one, near Menie, being a coarse crystalline rock, with a greenish tint, 8 × 5 feet. Another boulder of greenstone lies near it. Very frequently a stratum of red clay lies over the gravel ridges, encircling the base of boulders, indicating that after the gravelly ridges had been formed, and the boulders deposited, muddy sediment had been deposited in deep water. (Jamieson, "Geol. Soc. Journ.")

The following additional information sent by Mr James Mitchell, Boddam:—

No. 1 boulder, in a ravine at Bullers of Buchan, granite, 14 × 8 × 5 feet. About 15 feet above sea.

No. 2 boulder, on confines of Cruden and Peterhead. Granite, 18 × 12 × 5½ feet (above ground), 290 feet above sea.

No. 3, half a mile to E. of No. 2, a granite boulder, 13 × 9 × 5 feet, at a height of 260 feet above sea.

Along the south side of Peterhead Bay, and as far as Buchan Ness, the shore is strewed with blocks of granite, gneiss, trap, and sandstone; many of them belonging to rocks not found nearer than 20 or 30 miles.

A belt of gravel and calcareous sand forms a semicircular arc, with a radius of about 3 miles from the coast, passing through Crudens and Slains. The most conspicuous hillock in the line is a narrow Kaim in Slains parish, called the *Kipet Hill*,—the abode of fairies and elf bulls.

Compact groups of boulders form lines generally in a N.E. and S.W. direction. But a large number have been sown broadcast.

Culsalmond (Garioch).—Boulder of blue gneiss, $6\frac{1}{2} \times 2\frac{1}{2}$ feet, known as the Newton Stone, containing Ogham and other very antique inscriptions. (Professor Nicol in letter to Convener.)

Ellon.—At junction of Ythan and Ebrie, sienitic greenstone boulder, $22 \times 9\frac{1}{2} \times 8\frac{1}{2}$ feet, resting on gneiss. Near same place, another still larger. All these boulders have come from W. or W.N.W. (Jamieson, in letter to Convener.)

Glass (5 or 6 miles west of Huntly).—Five blocks called "*Clachan Duibh*" (Black Stones), on Tod Hill. Girth of each about 50 feet, and height from 10 to 12 feet. Being of same rock as hill, not certain whether brought from a distance. Other boulders on hill apparently different from adjoining rocks. Height above sea about 1000 feet. (Reporter—J. F. Macdonald, parochial schoolmaster.)

Kemnay.—Boulder, $38 \times 30 \times 10\frac{1}{2}$ feet, about 300 feet above sea; longer axis, E. and W. Boulder, $35 \times 30 \times 10$ feet, about 325 feet above sea; longer axis N. and S. Boulder, $25 \times 23 \times 8$ feet, about 325 feet above sea; longer axis, E. and W. Boulder, $28 \times 25 \times 8$ feet, about 325 feet above sea; longer axis N. and S. Boulder, $30 \times 28 \times 10$ feet, about 360 feet above sea; longer axis, N. and S. Boulder, $33 \times 27 \times 6$ feet, about 360 feet above sea; longer axis, N. and S. Boulder, $21 \times 20 \times 3$ feet. All these boulders are blue gneiss, whilst rocks adjoining are a coarse grey granite. On Quarry Hill, situated to north, 600 feet above sea, the rocks show striations indicating movement from west. Kaimes in valley parallel with valley running N.E. and S.W. for two or three miles. Legend, about devil throwing boulders at church from Bennachie Hill, situated to N.W. about eight miles. See ballad in Report. (Reporter—Rev. George Peter, M.A., parish minister.)

Logie Coldstone.—This parish thirty miles N.W. of Aberdeen. Surrounded at N.W. by amphitheatre of hills, of which Morven 2850 feet high. It contains numerous mounds of gravel and sand, in layers, showing action of water. They have the form of "kaims." Though there are no boulders, there are pebbles up to a *cwt.* or more, imbedded in water-worn gravel and fine sand. The pebbles are of same rock as adjoining hills—gneiss, granite, and hornblende. Two singularly shaped mounds, one 60 feet high, the other composed entirely of sand. They resemble the terminal moraines seen in the Grindelwald and other parts of Switzerland. Some years ago a number of boulders (from 3 to 6 tons in weight) were destroyed at a place situated to the north of this. They were of a soft, bluish granite, differing from any granite rock within a distance of nine or ten miles. One of these boulders might weigh 20 tons. This place had all the appearance of an ancient lake. The boulders may have been brought to it by same agency as that now seen on the Märjelin See, near Aletsch Glacier. (Reporter—J. G. Michie, school-house, Coldstone, Tarland.)

New Deer.—A great number of boulders, from 1 cwt. to several tons, lie in a sort of line for more than a mile S.E. from farm of Green of Savoch, as far, at least, as the hill of Coldwells and Toddlehills, in parish of Ellon. Elsewhere they are mostly on surface. Locally called "Blue Heathens." On Whitestone Hill, Ellon, and on Dudwick Hill, chalk flints are exceedingly abundant. (Reporter—James Moir, Savoch, by Ellon.)

In this parish formerly there was a rocking-stone, called "The Muckle Stone of Auchmaliddie." On the Hill of Culsh, formerly a Druidical circle. About seventy years ago the stones were carried away to aid in building a manse. Farm where situated still called, "The Standing Stones of Culsh." (Rev. J. Pratt's Account of Buchan, 1858.)

Towie.—Stone of unhewn granite, standing about 7 feet above ground, on north side of river Don, near bridge. Supposed to be Druidical ("New Statistical Account" of parish).

ARGYLL.

Appin.—Granite boulder 20 × 18 × 11 feet, about 290 tons. Differs from adjoining rocks. Longer axis N.E. Striated. Apparently has come from head of valley, which to N. or N.E. There is also a line of boulders;—rocks striated in direction of glen. (Reporters—James M'Dougall and Sir James Alexander, who sends a sketch.)

Ardentinny.—1. Boulder, called "Pulag"* (Big Round Stone), about 30 tons. In critical position on edge of cliff. 2. Boulder, called "Giant's Putting Stone," pear-shaped, and rests on small end. 3. Boulder, called "Clachan Udalain" (nicely-balanced stone), larger. (Reporter—Rev. Robert Craig.)

Duncansburgh (near Kilmallie).—Granite boulder, 7 × 5½ × 5 feet, called "Trysting Stone." Tradition. There are larger boulders nearer Ben Nevis. (Reporter—Patrick Gordon, min., *Q. S.* Duncansburgh, Fort-William.)

Dunoon (Kirn).—Trap boulder, 21 × 14 × 7 feet, about 164 tons. The adjoining rocks are mica schist and clay slate; striated. Photograph sent. (Reporter—Rev. James Hay, minister of Kirn.)

Glencoe.—Trap boulder, about 90 feet in girth and about 10 feet high. It is nearly round, and lies on an extensive flat, so that very conspicuous from a distance. (Reporter—Captain White, R.E.)

Inishail (North of Inverary).—Granite boulder about 8 feet above ground, called "Rob Roy's Putting Stone," about 1 mile from Taynuilt Inn on Oban road, about 60 feet above sea. A mountain of same rock about 1 mile distant. Longer axis, E. and W. Due west from above about 1½ miles, another boulder on a ridge on side of Loch Etive, in Muckairn parish. Several large boulders on road between Dalmally and Tyndrum; also on road between Tyndrum and Black Mount, about 4 or 5 miles from Tyndrum. A fine boulder on Corryghoil farm (Mr Campbell) between Inishail and Dalmally. (Re-

* Another translator states that "*Pulag*" in Gaelic means a "*dome.*"

porter—Rev. Robert M. Macfarlane, minister of Glenorchy and Inishail).

Inverchaolain.—Gneiss boulder, $10\frac{1}{2} \times 7 \times 5\frac{1}{2}$ feet, about 30 tons. Called "Craig nan Cailleach" (Old Wife's Rock). Differs from rocks of district. At head of Loch Striven, many boulders, same as rocks. (Reporter—John R. Thompson, schoolmaster, Inellan.)

Iona (Island).—Granite boulder, $24 \times 18 \times 6$ feet, 190 tons. Longer axis N.W. There are a great many others, chiefly on E.S.E. side of island, opposite to Ross of Mull, from which boulder supposed to have come. On other hand, Duke of Argyll is said to consider that the granite of the boulder is not the same variety as that of Ross. There are several boulders oddly placed near top of highest hill on N.W. side. (Reporter—Allan M'Donald, parish schoolmaster.)

Kilbrandon (Easdale by Oban).—On Lord Breadalbane's estate, grey granite boulders from 21 to 28 feet in girth, and standing from 3 to 4 feet above ground. Longer axis generally N.W. Ruts or grooves on tops and sides of some, bearing N.W. These boulders sometimes single, sometimes in groups, sometimes piled on one another. Occur at all levels from shore up to hill tops. No granite *in situ* nearer than Mull, which is 15 or 20 miles distant to N.W. (magn.) (Reporter—Alexander M'Millan, schoolmaster, Kilbrandon.)

Kilmallie.—Boulder, $12 \times 10 \times 10$ feet, about 100 tons. There is another, said to be larger, in the distant moors; also quartz boulder, about 9 feet square, supposed to have come from Glenfinnan, about 15 miles to N.W. by W. (Reporters—Rev. Arch. Clerk, and C. Livingston, schoolmaster.)

Kilmore and Kilbride (near Oban).—Granite boulder, 12 feet long; diameter of shortest axis, 5 feet; longer axis, E. and W. A few feet above sea mark. Adjacent rocks conglomerate. Another stone, about 200 yards distant, called "Dog Stone," of which photograph sent. It is a conglomorate. (Reporter—C. M'Dougall, Dunollie, Oban).

Lismore (Island of).—Boulders of granite, red and grey, lie on the limestone rocks of the island. An old sea terrace described, as encircling the island, on one part of which a cave, from the

crevices of which shells picked by Reporter (Alexander Carmichael, Esq., of South Uist, Lochmaddy, who refers also to the Rev. Mr Macgrigor, minister of Lismore).

Saddell (Kintyre).—Several small granite boulders, though there are no granite rocks in Kintyre. A good many whinstone standing stones. (Reporter—Rev. John G. Levach, Manse of Saddell.)

South of Campbelton, many granite boulders, like Arran granite, one near Macharioch, 4 × 5 × 2 feet. (Reporter—Professor Nicol, Aberdeen.)

At Southend, a boulder of coarse grey granite, about 18 feet in circumference, and weighing more than 3 tons, now broken up.

Another granite boulder, about 12 feet in circumference.

Two boulders of sienite, each 2 or 3 tons, about 200 feet above sea.

No granite rocks in neighbourhood. Rocks chiefly limestone and red sandstone. (Reporter—D. Montgommerie, Southend parish school.)

AYR.

Coylton. — Granite boulder, $11 \times 7\frac{1}{2} \times 5$ feet, about 30 tons. Longer axis N. and S. There are four more boulders, about 4, 8, and 12 tons. They form a line running N. and S. Legend, that King Coil dined on large boulder. (Reporter—Rev. James Glasgow.)

Dailly.—Granite boulder about 36 tons on Killochan Estate, called "The Baron's Stone." About 100 feet above sea. Lies on Silurian rocks. Apparently derived from granite hills situated S.S.E., near Loch Doon, about 13 miles distant. Boulder proposed to be blown up by tenant of farm. But old inhabitants interposed, and an inscription put on it by proprietor, Sir John Cathcart, in these terms, "The Baron's Stone of Killochan." Granite boulders of various sizes, on hill slopes, south of river Girvan. One on Maxwelton farm 800 feet above sea, contains 240 cubic feet. Another, 16 feet long, on top of Barony Hill above Lannielane, mostly buried under turf. Level mark on it by Ord. surveyors of 1047 feet above sea.

Doone Loch.—Two miles south of,—a granite boulder, about 25 × 20 × 12 feet, called "Kirk Stane." (Seen by Convener.)

Girvan.—Thousands of granite boulders for miles along shore near Turnberry Point, and some whinstones. Rocks *in situ* sandstone. (Reporter—Superintendent of Turnberry Lighthouse works.)

Along coast 4 miles south, in a ravine, two boulders of altered Greywacke. Largest, 17 × 13 feet, and weighs 180 tons. Other weighs about 100 tons. Have probably come from hills to S. or S.E.

Maybole.—Granite boulder, flat and oblong, on slope of hill above river Doon, on Auchindrane, at height of 230 feet, known as Wallace's Stone, from tradition, that a rude cross carved on it represents the sword of that hero. (These cases from Dailly, Girvan, and Maybole, communicated by Professor Geikie).

Banffshire.

Banff.—In district between Banff and Peterhead, beds of glacial clay, of a dark blue colour, very similar to beds in Caithness, and probably drifted from Caithness. Near Peterhead, many boulders of granite and trap. One of these, $4\frac{1}{2} \times 2\frac{1}{2} \times 1$ feet, a fine grained tough trap, of a greenish colour, not known *in situ* in Aberdeenshire, but occurs in Caithness. (Jamieson, "Geol. Soc. Jour.," xxii. p. 272.)

Boyndie.—Hypersthene boulders along shore, and found for some miles running S.W. Supposed to have come from rock to S.E., called "Boyndie Heathens." (Reporter—James Hunter, Academy, Banff.)

Fordyce.—A line of boulders can be traced running through parishes of Ordiquhill, Marnock, Grange, Rothiemay, and Cairney, in a direction S. and N. The boulders are a blue whinstone. In Ordiquhill parish, boulders, so close as to almost touch. They are called "Heathens." 500 feet above sea. (Reporter—Parish minister.)

Caithness.

Dunnet.—Conglomerate boulder of small size, apparently from "Maiden Pap" Hill, thirty miles to south. Several large

boulders in parishes of Olrich and Cannesby. (Reporter—Robt. Campbell, parish schoolmaster.)

Thurso.—Near Castletown, large granite boulder, which supposed to have come from Sutherland.* Between Weydale and Stonegun, several large conglomerate boulders.

Wick.—Three large boulders, differing from adjoining rocks, weighing from 20 to 60 tons. One is a conglomerate, apparently from mountains twenty miles to south.† (Reporters—John Cleghorn and J. Anderson.)

Granite boulder, 12 feet long, in drift, striated. Fragments of lias, oolite, and chalk flints, in same drift. Striations of rocks and boulders in Caithness indicate a general movement from N.W., *i.e.*, from sea.

Dumfries.

Kirkconnell.—Granite boulder, about 9 feet diameter, 20 to 30 tons; 700 feet above sea, called "Deil's Stone." Differs from adjoining rocks. Granite rocks in Spango Water, about three miles to north. (Reporter—R. L. Jack (Geolog. Survey).)

Tynron.—Three whinstone boulders, each weighing from 20 to 30 tons; also several conglomerate boulders. All have apparently come from N.W. (Reporter—James Shaw, schoolmaster, Tynron, Thornhill.)

Wamphray.—Large whinstone boulder. King Charles II. halted with his army and breakfasted here. (Reporter—Parish minister.)

Edinburgh.

Arthur Seat.—On west side of, boulders of limestone, supposed to have come from west. Rocks at height of 400 feet above sea, smoothed and striated in direction N.W.

Between Arthur Seat and Musselburgh, boulders smoothed and striated. Striæ run from N.W. and W.N.W. (Roy. Soc. of Ed. Proceedings, vol. ii. p. 96.)

* Rev. Mr Joass, of Golspie, states that granite occurs at a less remote locality.

† Rev. Mr Joass states that conglomerate rock occurs to the westward at a less distance.

Pentland Hills.—1. Mica-slate boulder of 8 or 10 tons. Supposed by Mr Maclaren to have come from Grampians, 50 miles to N., or from Cantyre, 80 miles to W., about 1400 feet above sea. 2. Greenstone boulder, 12 or 14 tons. Nearest greenstone rock *in situ*, 500 or 600 feet lower in level to N.W. 3. Sandstone boulder, about 8 tons, differing from adjacent rocks. (The above mentioned in Maclaren's "Fife and Lothians," p. 300.) 4. Greenstone boulder, about 10 tons, near Dreghorn. (Fleming's "Lithology of Edinburgh," p. 82.)

West Calder.—Whinstone boulder, 8 × 7 × 7 feet, about 28 tons. Adjoining rocks are sandstone. (Reporter—S. B. Landells, teacher.)

Elgin.

Dallas.—Numbers of small granite boulders found here, which supposed to have come from Ross-shire.

Duffus.—On Roseile Estate, conglomerate boulder called, "Hare, or Witch's Stone," 21 × 14 × 4 feet, longer axis N.W. Farm named "Keam," from being situated on a sandy ridge.

Elgin.—1. Conglomerate boulder on Bogton farm, 4 miles south of Elgin, 15 × 10 × 8 feet, about 80 tons. Longer axis is E.N.E., called "Carlin's Stone." Also a smaller one, called the "Young Carlin," to N.W. about half a mile. 2. Conglomerate boulder, 4 × 4 × 3 feet, about 3 tons. 3. Gneiss boulder, 13 × 8 × 6 feet, about 46 tons, called "Chapel Stone." Situated west of Pluscardine Chapel. 4. Sienite boulder, 12 × 8 × 3 feet, about 13 tons. 5. Sienite boulder, 8 × 6 × 2 feet, about 7 tons. The rocks *in situ* are all Old Red Sandstone. On Carden Hill, rocks smoothed and striated;—the direction of striæ N.W. (Reporter—John Martin, South Guildry Street, Elgin.)

Forres.—Conglomerate boulder, 9½ × 8 × 8 feet, about 44 tons, called "Doupping Stone." (Reporter—John Martin.)

Llanbryde, St Andrews.—Gneiss boulder, 15 × 9 × 7 feet, about 70 tons, in bed of old Spynie Loch, called "Grey Stone;" longer axis is N.N.E. and S.S.W. (Reporter—John Martin.)

New Spynie.—Four conglomerate boulders, lying on Old Red Sandstone rocks. (Reporter—John Martin.)

Rothes.—Six hornblende boulders, lying on gneiss rocks; dimensions and positions given. (Reporter—John Martin.)

FIFE.

Balmerino.—Mica schist (?) boulder, 12 × 9 × 8 feet; destroyed some time ago. (Reporter—James Powrie, Esq., Reswallie, Forfar.)

Crail.—Granite boulder, 10 × 8 × 6 feet, called "Blue Stone o' Balcomie," close to sea margin at East Neuk. Also trap boulder, 12 × 8 × 7½ feet. (Reporter—Captain White, R.E.)

Dunfermline.—Whinstone boulder, 17 × 15 × 6 feet, about 114 tons, called "Witch Stone." Legend. (Reporter—Robert Bell, Pitconochie.)

Leslie.—Kaim of sand and gravel near village, 100 to 300 feet wide, and 20 feet high, cut through by a brook. (Reporter—John Sang, C.E., Kirkcaldy.)

Newburgh.—On shore, near Flisk point, boulder of sienitic gneiss, about 15 tons. Legend is, that a giant who lived in Perthshire hills flung it at Flisk church. (Dr Fleming, "Lithology of Edinburgh," p. 83.)

West Lomond.—Hill about 1450 feet above sea, boulder of red sandstone and porphyry lying on carboniferous limestone. (John Sang, C.E., Kirkcaldy.)

FORFAR.

Airlie.—A remarkable kaim running two miles eastward from Airlie Castle. (Reporter—Daniel Taylor, schoolmaster.)

Barry.—Granite, sienite, and gneiss boulders and pebbles, on shore, and also on raised beaches, 11 and 45 feet respectively above sea level. (Reporter—James Proctor.)

Benholm.—Huge granite boulder, called "Stone of Benholm," now destroyed. Boulders on sea shore, of granite and gneiss, many of which are supposed to have come out of the conglomerate rocks, which occur here *in situ*. One boulder 18 × 12 × 3 feet, another 12 × 6 × 4 feet. "Stone of Benholm," stood on apex of a Trap knoll. The Trap knoll presents a surface of rock, which has apparently been ground down and smoothed by some agent passing over it from west; the exact line of move-

ment seems 10° to 20° south of west (magn.) In this Trap knoll there are agate pebbles, which have been mostly all flattened on west side, and been left steep and rough on east sides. Small hills which range in a direction north and south are scalloped, as if some powerful agent passing over them from westward had scooped out the softer parts. Hills ranging east and west, form a ridge with a tolerably level surface. Gourdon Hill and Craig Davie show marks of great abrasion. (Reporter—Rev. Mr Smart Myers, parish minister.)

Carmyllie.—Granite or gneiss boulder, from 7 to 10 tons. Differs from rocks near it. It lies on a height. Called "The Cold Stone of the Crofts." Supposed to have come from hills thirty miles to north. (Reporter—Rev. George Anderson.)

Cortachy.—Whinstone (?) boulder, 13 × 10 × 8 feet, about 78 tons Longer axis E. and W. Supposed to have come from a trap dyke situated to N.W. Legend, that thrown from N.W (Reporter—Rev. Geo. Gordon Milne.)

Mr Powrie of Reswallie reports a mica schist boulder as situated in South Esk river, about 60 or 80 yards below bridge, and within Earl of Airlie's park. Parent rock supposed to be 2 or 3 miles to N.W. This boulder probably same as that mentioned by Rev. Mr Milne.

Farnell.—Boulder $9\frac{1}{2} \times 7\frac{1}{2} \times 2\frac{1}{4}$ feet, about 12 tons. Supposed to have come from N.W. about thirty miles. (Reporter—Rev. A. O. Hood, parish minister.)

Inverarity.—Two grey granite boulders, from 2 to 5 tons each; destroyed some time ago. (Reporter—Rev. Patrick Stevenson.)

Kirkden.—Kaims, 440 paces long, running E. and W.; slope on each side from 22 to 30 paces; composed of gravel and sand. (Reporter—Rev. James Anderson.)

Kirriemuir.—A number of granite boulders in centre of parish, both grey and red. They lie chiefly between Stronehill and Craigleahill. Supposed to have come from Aberdeenshire.

Two kaims on Airlie Estate, one 100 yards long and 30 feet high, N.W. and S.E. on Upper Clintlaw Farm; other on Mid Scithie Farm, about 200 yards long and 30 feet high. At south base of Criechhill, a group of kaims, apparently

caused by confluence of great streams from N.E. and N.W. glens.

Old Red Sandstone rocks in S. of parish. Igneous rock towards N. at Craigieloch.

Slate rocks in Lintrathan and Kingoldrum. (Reporter—David Lindsay, Lintrathan, by Kirriemuir.)

Liff.—1. Mica schist boulder, 8 × 6 × 4 feet, called "Paddock Stone." Legend. Longer axis, N. and S. One report bears that it is whinstone, and may have come from Pitroddie Quarry, fourteen miles west. 2. Two boulders of mica schist, each 8 or 10 tons, called "Gows of Gowrie," noticed by Thomas the Rhymer. 3. A Druidical circle of nine large stones—three mica schist, one granite, five whinstone. Central stone, longer axis N. and S. (Reporters —James Powrie, Esq., Reswallie, Forfar; P. Anthony Anton, St Regulus Cottage, St Andrews.)

Menmuir.—1. Granite boulder, 14 × 9 × 4 feet, about 36 tons. Longer axis N. and W. Striated. Called the "Witch Stone." 2. Granite boulder, 13 × 9 × 4 feet, about 34 tons. There are many others smaller. (Reporter—Rev. Mark Anderson, Menmuir, Brechin.)

Montrose.—On Garvock and other hills, striæ on rocks point W. by N., *i.e.*, obliquely across the hills, which range W.S.W. and E.N.E.

On Sunnyside Hill, pieces of red shale found, derived from rocks *in situ* many miles to N.W. at a locality 100 feet lowest level.

Large blocks of gneiss, several tons in weight, occur, which must have come from Grampians, many miles farther to west. (James Howden, "Edin. Geol. Soc. Trans." vol. i. p. 140.)

Rescobie.—Mica slate boulder, 13 × 7 × 7 feet, near top of Pitscandly Hill, lying on drift. Rocks *in situ* Old Red Sandstone. Sir Charles Lyell says it came from Creigh Hill, about seventeen miles to W.N.W. Longer axis N. by E. 550 feet above sea. Valley of Strathmore lies between boulder and parent rock, and there are several hills also between boulder and parent rock, higher than boulder. Many smaller boulders of old rocks on same hill. (Reporter—James Powrie, Esq., Reswallie, Forfar).

St Vigeans.—Gneiss boulder, now destroyed. Supposed to have come from mountains situated to N.W. If so, it had to cross valleys and ridges of hills. Kaims in parish full of granite and gneiss boulders. (Reporter—Rev. William Duke, minister.)

Hebrides.

Barvas.—On Estate of Sir James Matheson, a monolith, called *Clach an Trendach*, or "Gathering Stone." Height above ground, 18 feet 9 inches, and girth 16 feet. (Reporter—Rev. James Strachan.)

Harris.—A large boulder on a tidal island, broken into two fragments, 100 feet apart. (Reporter—Alex. Carmichael.)

North Uist.—On a small island called Cāneum, north of Lochmaddy Bay, there are two boulders of Laurentian gneiss, which, though 100 feet apart, are evidently the two fragments of one block. The rocks *in situ* are also gneiss; but there is no hill or cliff near, from which the block could have fallen or come. One boulder weighs about 15, the other about 50 tons. They are both on the sea-beach, with a ridge or isthmus of rock between them. The boulders have each a side—in the one concave, and in the other convex—which face one another, and correspond exactly in shape and size. The edges of these two sides (viz., the convex and concave) are sharp, whereas the other sides in both boulders are rounded, suggesting that the original block had undergone much weathering or other wearing action before being fractured. The larger boulder rests fantastically and insecurely on two smaller blocks. Reporter thinks the boulder brought by ice, and that it fell from a height, and was split by the fall.

In Long Island the hills even to the summits are covered with blocks and boulders. As a rule the edges of these are sharp, whereas the native rock, whether low down or high up, is glaciated, grooved, and striated to a very remarkable degree. The best places to see these marks are where drift, covering them, has been recently removed. They are obliterated in the rocks, which have been much weathered. (Reporter—Alex. Carmichael, Esq., South Uist, by Lochmaddy.)

The Lewis.—(Q. S. Parish of Bernera. On farm of Rhisgarry, be

longing to Lord Dunmore.) Gneiss boulder, $8\frac{1}{2} \times 7 \times 3$ feet. Longer axis N. and S. 30 feet above sea. Striated N. and S. Striæ from 2 to 4 feet long. Same rock as those *in situ*. Called "Craig nan Ramh." (Reporter—Rev. Hugh Macdonald, Manse, Bernera.)

The Lewis (Stornoway, Tolsta).—A rocking stone of gneiss $12 \times 5 \times 4\frac{1}{2}$ feet. Longer axis N.W. and S.E. About 200 feet above sea. Rocks *in situ* also gneiss. There are boulders of trap, apparently brought from eastward, where there are trap dykes. At a corner of a rocky hill near Tolsta, there are huge pieces of rock lying, suggesting idea of having been broken off by an iceberg. On Park Farm, beside a loch, there is a solitary boulder. Near Stornoway Tile Works, a boulder of Cambrian rock, supposed to have come from mainland to eastward. (Reporter—Mr Peter Liddell, Gregs, by Stornoway.)

Stornoway.—Several boulders occur near Garabast, of a rock similar to that which exists at Gairloch, on mainland to east (about 35 miles across the sea). There is also a large standing stone at Paible. (Reporter—Henry Caunter, Esq., Stornoway.)

In Forest of Harris, and beween Fincastle and Glen Ulledale, there are many evidences of (supposed) ice action, viz., rocks smoothed and striated, and boulders lying in lines. (Reporter—Capt. Thomas, R.N.)

Report by Mr Campbell of Islay.

The well-known author of "Frost and Fire," who has studied the subject of the transport of boulders, not only in Scotland, but in many foreign lands on both sides of the Atlantic, has sent to the Committee a report, from which the following extracts are made:—

"I find in Scotland, upon ridges which separate rivers, marks of glaciation upon a large scale. These enable me to say, with tolerable certainty, that the ice which grooved rocks in the Outer Hebrides, at low levels, near sounds, moved from the ocean in the direction which tides now follow in the straits beside which the striæ are found.

"The conclusion at which I have arrived, by the examination of all these phenomena, boulders included, is, that a system of glaciations prevailed in Scotland, which can be ex-

plained by the system now existing in Greenland. There, a vast system of Continental ice, as great in area as all India, radiates seawards, and launches icebergs, which move about in tides and currents. This system certainly existed in Scotland previous to the smaller system.

"Following any glen in Scotland, say Glenfyne, the smaller system of glaciation follows the course of the river (as in Switzerland), and the course of the tides in the sea loch (as glaciers do in Greenland); and, furthermore, often overruns low watersheds, and runs out to sea in some direct line. The striæ which mark the run of ice from the head of Glenfyne to Lochgilphead, run over a col and down Loch Killisport. They run past Tarbert, down both sides of Ceantyre and Arran, and out to sea. At Ormsary, by the roadside, and on the sea-beach, is a train of large boulders to which the usual legends are attached. One was thrown from Knapdale at a giant who was eating a cow on the other side of the loch. One of these boulders close to Ormsary House, at a small roadside cottage, is the biggest I have seen in Scotland. I did not try to ascertain whence it came. I think it was pushed a short distance only. But the striæ and trains of blocks show that it moved from N.E. to S.W. along the general line of hollows in the Western Highlands.

"On the outer islands in Scotland are marks equivalent to those so conspicuous on shore. In the Long Island, from Barra Head to the Butt of Lewis, the whole country glaciated, and the boulders everywhere perched upon the hills. Where surface newly exposed, the striations and smooth polishing so perfect and fresh, that marks can be copied as *brasses* are copied in churches by antiquaries. I showed to you samples taken last year in Barra and Uist. I have a large series taken wherever I have wandered. These enable me to say, with tolerable certainty, that the ice which grooved rocks in Outer Hebrides at low levels, near sounds, moved from the ocean in the direction which tides now follow in the straits, beside which the striæ are found. For example, the grooves upon the flat at Iochdar, at the north end of South Uist, aim directly at the Cuchullin Hills in Skye. At the Mull of Ceantyre, at a

great height above the sea, grooves aim at Rhinns of Islay parallel to the run of the tides. And so it is at a great many other places all round the coast."

In a letter from the same gentleman to Mr Carmichael, of South Uist, dated 29th March 1872, the following passages occur:—

"Glacial striæ occur upon fixed rocks in Tiree, Minglay, Barra, South and North Uist. They correspond with a direction from the N.W., or thereabouts.

"The striæ abound, and are especially fresh in the low levels, and opposite to hollows in hills, which would be under water, and traversed by tides, if those levels were now to sink a few hundred feet. The hills, so far as I have examined them, are ice-worn to the very top. Transported blocks are scattered all over these islands. In some places regular boulder-clay is left in patches. Under the clay, the rocks are smooth as polished marble. The boulders, so far as I have been able to ascertain, are of the same rock as the rock of the islands named. Boulders in Tiree, for example, may have come from Uist or Barra. They are perched upon the highest hill-top in Tiree.

"I was unable to find any sample of the rocks of Skye in Uist or in Tiree."

INVERNESS.

Kilmallie.—Boulder, fully 2000 feet above sea, on summit of a hill, 12 × 10 feet. Another still larger among the mountains between Loch Shiel and Loch Arkaig. Also boulder drifts and moraines in numbers. (Reporter—Rev. Archibald Clerk, Kilmallie Manse.)

Kilmallie (near Ardgour).—Quartz and mica boulders, nearly round, and remarkable on bare hill side. Different from adjacent rocks. 110 feet above sea. Same kind does not occur nearer than Glenfinnan, situated fifteen miles to N.W. by W. (Reporter—C. Livingston, parochial schoolmaster.)

Kilmonivaig (Glengarry, N.W. of Fort William), Estate of Edward Ellice, M.P.—Boulder on Monerrigie Farm, near Lochgarry, about 16½ feet long at base, and 23 feet at top, and about 9 feet high. Round at top. Quartzite rock. No rock ***in situ*** near.

Longer axis N. and S. Several boulders on Leek·Farm, near Loch Lundie, considerably larger. Some of boulders examined by Mr Jolly, school inspector, Inverness, and found by him to be striated. On Faicheam Ard Farm boulders very peculiar, being entirely different from all rocks in neighbourhood. Have been objects of curiosity to many geologists. The boulders generally arranged in groups, except at Faicheam Ard, where piled on one another. They rest on gravel. At Leek, near Iron Suspension Bridge, rocks *in situ* well striated.

There are "kaims" in another part of parish. At mouth of Glengarry a delta of fine gravel. In Lochaber also, along banks of Spean and Lochy. (Reporter—Parochial Schoolmaster.)

Kiltarlity (on Lord Lovat's lands).—A group of boulders called whinstones. Rock of same kind "a little southwards." Dimensions of two largest are (1.) 15 feet long, 9 feet high, 10 feet broad; (2.) 8 feet long, 6½ feet high, 13 feet broad. Longer axis of both E. & W. Angular in shape. Several natural ruts on both 4 or 5 feet long, running N.W. About 300 feet above sea. (Schoolmaster's schedule, but omitted to be signed.)

Kingairloch (near Fort William).—Boulder, 5 × 5 × 4 feet, about 5 tons; 8 feet above sea. Different from adjacent rocks. (Reporter—D. Cameron, teacher.)

Kingussie.—Boulder of a slaty rock, 15½ × 12 × 9, about 120 tons. Longer axis, E. & W. Called "Fingal's Putting Stone." About 900 feet above sea. Several other large boulders near Laggan Free Church. (Reporter—Cluny M'Pherson, Cluny Castle, Kingussie.)

Lochaber.—Near summit of Craig Dhu, between Glens Spean and Roy, a black sienite boulder, 14 × 8 × 4 feet. On same hill lower down, boulders of red granite and felspar. (Observed by Professor Nicol and Mr Jamieson of Ellon. Mr Jamieson states that parent rock is in Glen Spean, to S.E. of Craig Dhu, and at a level far below boulders.) ("Lond. Geol. Soc. Journal," Aug. 1862 and Aug. 1863.)

On second Glenroy shelf, near the "Gap," a boulder of sienite, 8 × 7 × 4 feet. (Reporter—Professor Nicol.)

Morvern (near Fort William). — Grey granite boulder, called "Clach na'm Buachaillean." Length—North side, 17 yards; south side, 7½ yards; 17 yards "round about;" 13 yards "round top from ground to ground;" 11½ yards "across middle from ground to ground." A large boulder to east of above on a hill about 2640 yards distant, and "peculiarly laid upon other smaller stones." (Schoolmaster's schedule, but omitted to be signed.)

Kincardine.

Banchory.—On property of John Michell, Esq. of Glessel, not far from Glessel Railway Station, a boulder called the "Bishop's Stone;" circumference 44 feet, height above ground 8 feet, estimated to weigh 70 tons; bluish granite, differing from adjoining granite rocks. An ancient stone circle of boulders about 200 yards distant. (Reporter—Sir James Burnett of Crathes.)

The hill of Farre, situated two miles to north, forms an elongated range, running E. and W. Rocks on it glaciated, the striæ running about E. and W., *i.e.*, nearly coincident with valley of Dee. (Reporter—Thos. F. Jameson, Ellon.)

Fettercairn.—No boulder now left in parish, of any size. Long banks of gravel and sand occur, running parallel to one another. (Reporter—A. C. Cameron, parish schoolmaster.)

Maryculter.—Boulder, 5½ × 6 × 6 feet, about 14 tons. Longer axis N. and S. Rock of boulder considered same as rock situated to eastward. (Reporter—David Durward.)

Kirkcudbright.

Galloway.—A great accumulation of blocks at head of Loch Valley at Loch Narroch. Among these are blocks of the peculiar graphic granite of Loch Enoch to the north, so that these blocks must have been carried from Loch Enoch southwards into the basin of Loch Neldricken, on to the spur of Craignaw between it and Loch Valley, and still onwards right over Craiglee and its deep scooped lake basins into Glen Trool. Craiglee is remarkable for the number of perched blocks, some of immense size, scattered over its ridges and highest peaks.

The many boulders along its ridgy crest give the appearance of an old broken-toothed saw.

Throughout the whole region travelled blocks and boulders occur, even to the summit of the Merrick, the highest peak south of the Grampians (2764 feet). One set of perched blocks is interesting, viz., poised blocks, known as Rocking Stones. Such blocks are natural, and have been placed by no human hands. Their exquisite balance is the result of the weathering of the block and of the rock below, caused by wind and storm.

There are well-marked striated rock surfaces more than 1600 feet above the sea-level.

Various moraines described, as stretching across valleys like ramparts, and forming dams to existing lakes. (William Jolly in "Edin. Geol. Soc. Trans." i. 155.)

Kells.—On Craigenbay Farm, a grey whinstone boulder, about 10 feet high and 17 feet long, with girth of 54 feet; 800 feet above sea. Longer axis N. and S. (Reporter—Robert Wallace, Auchenbrack, Tynron.)

Kirkbean.—Grey Granite boulder, $16 \times 9\frac{1}{2} \times 7\frac{1}{2}$ feet, and girth about 38 feet, weighing about 80 tons. On sea shore at Arbigland. Longer axis, S.E. by E. Superficial groovings on top and S.W. front running N.N.W. Rests on freestone.

Criffel is about 3 miles to N.N.W. Granite rock there same as boulder. In all the glens, between sea shore and Criffel, numerous granite boulders generally in lines parallel with glens. Several kaims 40 to 50 feet high, run from $\frac{1}{4}$ to $\frac{1}{2}$ mile. (Reporter—Rev. James Fraser, Colvend Manse, by Dalbeattie).

Penninghame.—Granite boulders chiefly, supposed to have come from Minnigaff Hills, situated to N.E. Larger boulders on watersheds between Lochs Dee and Troul. (Reporters—Rev. William M'Lean, parish minister, and Rev. George Wilson, F.C. minister.)

Twynholm.—Granite boulder, supposed to have come from Galloway Hills, six or seven miles to westward. Several Druidical circles. (Reporter—Rev. John Milligan, Manse of Twynholm.)

LANARK.

Carluke.—Sandstone boulder, 20 × 14 × 14 feet, about 290 tons. Called "Samson's Sling Stone." Doubtful if an erratic. (Reporter—D. R. R.)

Carnwath.—Whinstone boulders in large heaps. Supposed to have come from "Yelpin Craigs," three or four miles to north. Legend about Michael Scott and witches. (Reporter—Rev. Mr M'Lean.)

NAIRN.

Auldearn.—A great many boulders in this parish, of old rocks, and lying chiefly on Old Red Sandstone rocks. Chiefly conglomerates, and apparently derived from same kind of rock, characterised by pebbles in it of angular quartz or hornstone, liver coloured. These boulders all lie on sides of hills facing N.W., and they have generally one of their sides smooth which fronts the west. (Reporter—James Rennie, schoolmaster.)

Ardclach.—At Raemore Burn, about 270 feet above sea, and 5 miles distant from sea, a conglomerate boulder with five sides, measuring altogether about 17 yards, and 3 yards above ground. Surrounded by hills of no great height; but lowest of these is to N.W. Fragments in conglomerate of quartz, hornstone, sienite, felspar, and other very hard rocks. The block is scarcely rounded at its edges and corners. (Reporter—Dr Gregor, Nairn.)

Cawdor.—On hill of Urquenay, the following boulders—1. At top of hill, about 690 feet above sea, conglomerate called "*Clach na Gillean,*" or "*Young man's stone,*" in girth about 54 feet, and height 10 feet. It rests on bare granite rock. 2. Halfway down hill, about 580 feet above sea, conglomerate called "*Clach na Cailleach,*" or "*Old wife's stone,*" in girth about 54 feet and height 15 feet. It seems to rest on drift gravel. 3. At foot of hill, and at east end of a kaim of gravel and sand, about 300 feet above sea, conglomerate called "*Clach an oglach,*" or "*Boy's stone,*" in girth about 69 feet, and average height about 9 feet.

Within policy woods of Cawdor Castle, on side of a burn

facing W.N.W., a conglomerate boulder about 250 feet above sea, in girth about 100 feet, and about 12 feet high.

The above four conglomerate boulders lie on granite rocks.

On Piper's Hill, where rocks *in situ* are Old Red Sandstone, a conglomerate boulder, on the side of a kaim facing N.W., weighing about 10 tons. Above sea about 300 feet.

No conglomerate rock of the same hard description in Nairnshire. On the granite rocks there lie boulders of sandstone, evidently transported from the north, where the Old Red Sandstone only exists, in the low country. (Reporters—W. Stables, Esq., commissioner; and his clerk, Mr John Grant, Cawdor Castle.)

Croy.—Conglomerate boulder, called "Tomreach," about 15 feet high, and girth of 27 yards. About 300 or 400 feet above sea. Sketch sent. (Reporter—Captain White, R.E.)

Orkney and Shetland.

Bressay (Shetland).—A number of boulders consisting of a coarse white sandstone at various heights, viz., from 40 to 360 feet above sea. They lie on east side of island, and are conjectured to have come from Norway. Largest boulder, 10 × 7 × 4 feet. Longer axis, N.W. Distinct groovings N.E. and S.W. (true); some of them 3 inches deep. (Reporter—Schoolmaster?)

Eday (Orkney).—Conglomerate boulder, 12 × 7 × 1½ feet, about 8 tons. Longer axis N.E. Situated near top of hill, about 250 feet above sea. Called "Giant Stone." Legend, as to it being thrown from island of Stronsay. No conglomerate in Eday, but there is in Stronsay. (Reporter—G. Miller, schoolmaster, Cross and Burness.)

Frith and Stennis (Orkney).—Pebbles of white freestone on the hills. No white freestone rock in district; all red sandstone. (Reporter—Robert Scarth.)

Housay Island (Shetland).—On a cliff, 200 feet above sea, there are loose blocks resting on rounded knolls and polished rock, all polished before the burthen they now bear was thrown upon them. Some of the stones hang on ridges on the rounded sides of the hill.

Lerwick (Shetland).—At Lunna, a large block, broken into two, called the "Stones of Stoffus," but uncertain whether erratics. (Reporters — James Irvine, teacher, and Robert Bell, proprietor.)

North Unst.—Here ice action plain. The serpentine rock has suffered severely. Ruts and striæ on it W.N.W. A hill 500 feet high, whole of upper part of which for 150 feet from top polished. Striated stones and blocks also plentiful. All over Unst the rocks show signs of abrasion, and in many places deposits of drift, inclosing stones of all sizes, some of which are rounded and striated.

In the *Island of Ueay*, large perched blocks, some many tons in weight, lie scattered about everywhere.

Thus then, at both ends, and in the middle of this group of islands, traces of glacial action have been found. (Peach, Brit. Assoc. Rep. 1864.)

Sanday (Orkney).—Gneiss boulder, $7 \times 2\frac{1}{2} \times 6$ feet, about 14 tons. Rocks of island are Old Red Sandstone. At Stromness, thirty miles to S.W., gneiss rocks occur *in situ*, also in Shetland Islands to north. Legend, that thrown from Shetland. (Reporter—G. Miller, schoolmaster, Cross and Burness.)

Sumburgh Head (Shetland).—Conglomerate boulder, lying over sandstone. (Reporter—William Lawrence, teacher.)

Walls (Orkney).—Lydian stone boulder, $9 \times 7 \times 6$ feet, about 28 tons. Large quantities of granite boulders scattered over hills; valleys show glacier and iceberg agency. (Reporter—James Russell, teacher.)

Peebles.

Kirkurd.—Three boulders of gneiss or trap (?) differing from adjacent rocks. (Reporter—James Palmey, schoolmaster, Kirkurd, Dolphinton.)

Newlands.—Remarkable kaims. (Reporter—E. Blacklock, schoolmaster.)

Perth.

Aberfeldy (Tullypowrie village). 1. On north side of village, a considerable assemblage of schist boulders, the rocks *in situ* being clay slate. Most of boulders round in shape as if rolled.

One large boulder angular, $16 \times 14 \times 7$ feet, named "Clach Chinean," or "Stone of Doom." These boulders all rest on heaps of drift, much resembling a moraine. On the opposite or south side of the valley there are similar masses of drift, containing, however, stratified beds of sand and gravel.

2. About 2 miles north of Tullypowrie village, near the hills, two very large boulders of mica slate occur, about 1500 feet above sea. They rest apparently on a heap of drift. They are both cubical in form, and with sharp angles, as if never exposed to friction. One of them measured, and found to be 71 feet in girth and 17 feet high. The hills are more than $\frac{1}{4}$ mile distant. They must have been brought by ice of some kind, and let down without violence; for a fall from any height would have probably caused such large masses to break in pieces. The adjoining hills form a range to N. and W., reaching fully 700 feet above the boulders. But to N.W. (magn.) of the boulders, and within a $\frac{1}{4}$ mile a passage occurs through the hills, the level of which is only about 200 feet above the boulders. They might have come through this passage, carrying the boulders and stranding them where they now lie. These boulders, called "Clach M'had," or "Stones of the Fox."

3. Above Pitnacree House, a boulder of schist resembling hypersthene, $15 \times 11\frac{1}{2} \times 4$ feet above ground. It is called "Clack odhar," or "Dun Stone." No hills are near it, and it differs from all rocks *in situ* near it. (Reporter—Mr M'Naughton, merchant, Tullypowrie).

Arngask.—Rocking stone of mica slate, in Glenfarg ("New Statistical Account," vol. x. p. 888).

Auchterarder.—Boulder, $10 \times 6 \times 2$ feet, about 8 tons. Longer axis N.W. Called "Wallace's Putting Stone." (Reporter—Rev. Dr Nisbet, Edinburgh.)

Auchtergaven.—Granite boulder, $10 \times 8 \times 3$ feet, about 8 tons; 260 feet above sea. Longer axis N. and S. Called the "Deil's Stone." Has numerous and distinct "cup" markings on its sides. Supposed to have come from mountains situated thirty miles to north. Has been mutilated by slices cut off it for building, &c. Several standing stones and Druidical circles in

this parish, composed of boulders. (Reporter—William Duff, schoolmaster.)

Bendochy.—Formerly a Druidical circle of nine large stones, now destroyed, but name still preserved of "Nine Stones." Long kaims of gravel or sand, which supposed may have caused river Tay to fall into sea at Montrose. (Reporter—Rev. Dr Barty.)

Callendar (Stirling).—Gneiss boulder on top of Bochastle Hill, called "Samson's Putting Stone," 14 × 9 × 9 ft., resting on conglomerate rock. Longer axis N.E. Sketch sent, showing unstable position. Has come from westward. (Reporter—J. B. Hamilton, Leny.)

Collace.—Large stones said to be here. Query,—are they erratics? (Reporter—Peter Norae, schoolhouse, Collace.)

Comrie.—Four boulders of whinstone, and one of granite, 13 × 9 × 7½ feet, weighing about 20 tons. Longer axis N. and S. (Reporter—Wm. F. Swan.)

Crieff.—1. Conglomerate boulder, 16 × 10 × 5½ feet, about 64 tons, "Witches' Stone." 2. Conglomerate boulder, 19 × 10 × 5 feet, about 70 tons. 3. Red granite boulder, 8½ × 4½ × 4 feet, called "Cradle Stone." (Reporter—Rev. Dr Nisbet, Edinburgh.)

At Abercairney, dark grey granite boulder, about 20 tons. (Reporter—C. Home Drummond Moray; and Rev. Thomas Hardy, parish minister.)

In Glen Turret, appearances of ancient moraines, described in letter by Mr Sang, C.E., Kirkcaldy.

Doune (near Kilbride).—Conglomerate boulder, about 900 tons. (Described in Estuary of Forth, by Mr Milne Home.)

Dron.—Whinstone rocking stone, 10 × 7 feet. Stands on bare rock ("New Statistical Account," vol. x. 364).

Errol.—Several boulders, differing from adjacent rocks. Said to be indicated on Ordnance Survey maps.

Fortingall.—Gneiss boulder, 24 × 16 × 13 feet, called "Clach an Salaine," from people who brought trees out of Black Wood of Rannoch, resting them on it. Height above sea 2500 feet. Rocks *in situ* clay slate. Longer axis N.W. (Reporter—Mr Fletcher Menzies.)

Fowlis.—Two dark grey granite boulders, $10 \times 7 \times 4$ feet, and $12 \times 6 \times 4$ feet. Supposed to have been used as places of worship or sepulture, in very ancient times. (Reporter—Rev. Thomas Hardy.)

Killiecrankie (Tennandry Parish).—Blue limestone boulder, $6 \times 5\frac{1}{2} \times 4$ feet. Supposed to have come from "*Ben y Gloe,*" a hill to N.N.E., across valley 500 feet deep; plan of district sent. Granite boulder, also mentioned; has come from North. (Reporter—Rev. Patrick Grant, Tennandry Manse.)

Kilspindie.—Seven granite boulders, from 5 to 6 tons weight. Five form a belt or row having N.W. direction. All differ from adjacent rocks. (Reporter—James M'Kerracher, schoolmaster, by Errol.)

Kirkmichael.—Rocking stone, $7 \times 5 \times 2\frac{1}{2}$ feet, about 3 tons, whinstone. (?) Several tall stones near it, called "Clachan Sleuchdaidh" (Stones of Worship).—("New Statistical Account," vol. x. p. 737.)

Logie Almond.—Whinstone boulder, 8 or 10 feet square, about 48 tons, called "The Ker Stone," about 600 feet above sea, on north bank of River Almond, opposite to Glenalmond College. Probably, as there is a great peat moss near, the name has reference to the moss, "char" being the Gaelic for peat.

There is another boulder called "Cul na Cloich," or *Stone Nook.* A stream forms a nook or angle with the drain or ridge on which the boulder stands. It is a conglomerate, and rests on Old Red Sandstone. Another conglomerate boulder occurs at S.E. corner of the farm of Risk. (Reporter—Rev. Patrick Macgregor, Logie Almond Manse.)

Methven (Auchtergavin Parish).—Whinstone boulder, about 10 feet high, oval shaped, standing on small end, called "Sack Stone." No rock of same kind near. 800 feet above sea. (Reporter—William Duff, schoolmaster.)

Monzie.—In Glen Almond, a large stone, 8 feet high, near side of river, nearly cubical, called *Clach-Ossian,* said to mark grave of that poet. ("New St. Acct." of parish, vol. x. 264.)

Pitlochrie.—1. On road to Straloch, mica slate boulder, called "*Gledstone,*" about 1800 feet above sea. Lying on drift of gravel and stratified sand. Rocks adjoining clay slate.

About 8 tons weight. Legend, that this stone gave name to Gladstone family, an infant having been found at it by a shepherd, who took it home to his wife, who nursed it.

2. Near parish church of Straloch, a huge boulder of very coarse granite, called "*Clach m'hor*," or "*Big stone*," about 24 feet diameter, and about 20 feet high. Supposed to weigh about 800 tons. Adjoining rocks clay slate. Many other boulders of mica slate and quartzite beside it. Supposed to have come from north through a valley. (Reporter—Rev. Dr Robertson, Straloch.)

Rattray.—Mica schist boulder, 12 × 6 × 6 feet, about 25 tons, called "Glenballoch Stone." Has cup and groove markings on south side. There are other boulders in Druidical circles. They have all come from hills to N. or N.W. (Reporter—Rev. Mr Herdman, Rattray.)

Renfrew.

Kilbarchan.—Porphyry boulder, 22 × 17 × 12 feet, about 300 tons. Longer axis E. and W., called "Clach a Druidh" (Stone of Druid)? Legend. Boulder differs from adjacent rocks. Same rock seen in hills 2 or 3 miles to west and north. (Reporters,—Robert Graham, D.D.; and R. L. Jack (Geol. Survey).)

Ross and Cromarty.

Alness.—In forest of Gildermoy, a very large granite boulder reported by Earl of Selkirk.

Applecross.—Three large boulders, one near shore at Rassel, called "*Clach Oiu*," weighing about 60 tons, other two about 30 tons, each called respectively "*Clach Mhoir*" and "*Clach Van.*" Used as landmarks from the sea. Kaims at Ardbain and Ardrishach, extending each more than two miles along coast. (Reporter—William Ross, schoolhouse, Applecross.)

Ben Wyvis.—N.W. shoulder of, presents whole acres of rock, swept bare of soil, rounded and polished. Boulders of a peculiar veined granite have come from the Derry More (tract situated to west of Ben Wyvis), and been carried eastward to Moray Frith. These boulders found half-way up Ben Wyvis, also in valleys of Alness and Ault Grand. In Strathgarve some of

the blocks are as big as cottages. Their size lessens towards E. No boulder of same kind seen on West Coast. (Nicol "Geol. of N. of Scot.," p. 70.)

Carnock.—Five large boulders, each weighing about 20 tons. Each has a Gaelic name. One, a boundary stone. (Reporter—James Watson, schoolhouse, Strathconon, Beauly.)

Edderton.—Granite boulder, 23 × 19 × 12 feet, weighs about 290 tons. Longer axis N.E. Two others, not quite so large. All differing from adjacent rocks. (Reporter—Rev. Ewen M'Ewen, parish minister.)

Rev. Mr Joass states that this word is derived from "*Garbh*"—"*rough*," the Gaelic for "*Hill of the Pitcher*," on account of shape, its sides being almost vertical. (Rev. Mr Joass.)

Rev. Mr Joass of Golspie states, that the boulders here referred to are on a shelf or terrace about 900 feet above sea, and that their parent rock is at Carn na Cuinnaig about 12 miles to N.W.

He adds, that the boulders specified, as in the parishes of Tain and Tarbat, are probably from same source. The granite is peculiar. (See Tain and Tarbat farther on.)

Fannich Mountains.—Boulder of grey gneiss, with garnets. 30 × 10 × 5 feet, described in letter to Convener by J. F. Campbell of Islay; 2700 feet above sea; angular. Situated on watershed. Called "Clach mhor na Biachdoil." A train of large boulders to be seen in a valley not far off. Rocks also smoothed and striated. Lines of striation parallel with valleys.

Foddarty.—Boulder, 14 × 8 × 5 feet, about 40 tons. About 6 feet above sea; shape, angular; Druidical. Another with inscription illegible. Supposed to commemorate a battle between two clans. (Reporter, parish schoolmaster.)

Lochalsh.—Gneiss boulder, 9 × 7 × 8 feet; longer axis E. and W., striated. Boulder differs from adjacent rocks. Same rock said to be at Glenelg, 5 or 6 miles to south.

Boulder called after Fingal. Quartz, 7½ × 7 × 5 feet. Longer axis, N.W.; striated. At Loch Carron, said to be a kaim or diluvial bank. (Reporter—Duncan Sinclair, parish school, Lochalsh.)

Lochgair.—One granite boulder, 28 × 17 × 16 feet, about 560 tons striated. Two granite boulders, 23 × 10½ × 7 feet, about 120 tons. One of these said to be on top of a hill, and called "Sandel Stone." Legend. There are three other boulders of smaller size. Rocks *in situ* are granite. (Reporter—John MacKillop, schoolmaster.)

Shieldag (Loch Carron).—Granite boulder, 16 × 10 × 10 feet, about 120 tons. Longer axis E. and W. There is another large boulder. Both said to be in precarious positions. (Reporter—Rev. Alex. C. M'Intyre, Shieldag Manse, Dingwall.)

Tain.—Granite boulder, 18 × 12 × 8½ feet, about 60 tons. Plan and section of boulder given. Rocks of district are Old Red Sandstone. South shore of Dornoch Frith said to be thickly strewed with granite blocks, whilst none on north shore. (Reporter—Robert Gordon.)

Tarbat.—Seven or eight large boulders of gneiss and granite. Places, dimensions, and names specified, with sketches of boulders. Also, kaims of clay running E. and W. in parallel lines. One a mile long. (Reporter—Rev. George Campbell, parish minister.)

West Coast.—Vestiges of moraines, lateral and terminal, from glacier generated in valley occupied by Loch Fuir, N. of Loch Maree. (Nicol "Geol. Soc. Jour.," xiv. p. 170.)

Roxburgh.

Eckford.—Two kaims, each from 100 to 300 yards long, from 50 to 60 feet high. (Reporter—Parish schoolmaster.)

Jedburgh.—Porphyry boulder, supposed to have come from Dunion Hill, which is 2 miles to west. Formerly granite boulder on Dunion. Supposed to have come from Galloway or Dumfries now destroyed. A whinstone boulder, above Bedrule Bridge. (Reporters—Rev. Archibald Craig and Rev. Dr Ritchie.)

Melrose.—Greywacke boulder, round shaped, called "Samson's Putting Stone." (Reporter—Parish schoolmaster.)

Stirling.

Alloa.—Basaltic boulder, 13 × 11½ × 11 feet. Longer axis N. and S. Called "Hair Stane." About 70 feet above sea. (Reporter—Parish minister.)

Campsie.—Rocks glaciated. Striations W.S.W. & W.N.W. (Reporter—Rev. Thomas Monro, D.D.)

Fintray.—Boulders in a group, called "Gowk Stones." Have apparently come down valley. (Reporter—R. L. Jack (Geol. Survey).)

Kilsyth.—Mica Slate boulder, 7 × 5 × 2½ feet, about 6 tons. 1250 feet above sea. Parent rock supposed to be 15 miles to north. (Reporter—R. L. Jack (Geol. Survey).)

Ochils.—On watersheds of, at about 2000 feet, boulder of mica schist fall of garnets, apparently from Grampians to N.W. (Jamieson, "Geol. Soc. Jour.," xxii. p. 166.)

St Ninians.—Boulder about 200 tons, at height of 1250 feet above sea. (Reporter—R. L. Jack (Geol. Survey).)

Strathblane.—Conglomerate boulder, 8 × 4 × 3 feet, about 7 tons. Longer axis W. 20° N. 1803 feet above sea. Parent rock supposed to be to N.W. (Reporter—R. L. Jack (Geol. Survey).)

SUTHERLAND.

Assynt.—Two arge boulders, one at Unapool, the other at Stronchrubie, called "Clach na Putain" (Stone of the Button). (Reporter—Angus M'Ewen, parochial schoolmaster.)

Clyne.—Remarkable kaims, apparently moraines (lateral and terminal) in valley of Brora. Also, rocks striated at Brora quarry. Striæ run N.W. (Reporter—M. Myron.)

Golspie.—Old Red Sandstone boulder, 16 × 10 × 4 feet, lying on Oolite rocks. Longer axis, N.N.W.; sub-angular. Sketch sent. About 248 feet above sea. Three smaller boulders of Old Red Sandstone lie about 100 yards to S.E. of the above. The Old Red Sandstone formation is situated to north and west, about 3 miles from boulder. Terminal and lateral moraines occur in Brora valley, broken up by diluvial action into ridges and hummocks. (Reporter—Rev. James Joass, minister of Golspie.)

On the whole N.W. coast from Cape Wrath southwards, numerous "Perched" boulders occur on summits and sides of hills, in the most exposed positions. Especially numerous around Loch Maree. (Nicol "Geol. Soc. Journal," xiii. pp. 29, 39.)

Boulders of large size on top of Applecross Hills. Rocks below, striated. Direction of striæ S. 20° W. (true.) (Reporter—Nicol of Aberdeen.)

WIGTOWNSHIRE.

Glasserton.—Granite boulder, 9 × 6 × 6 feet, about 24 tons. Longer axis N.E. Two small boulders to east of above, and in a line with it. These boulders supposed to have come from mountains to N.E., across arm of sea. Kaims in parish, full of granite pebbles. (Reporter—Archibald Stewart.)

The following Gentleman was elected a Fellow of the Society:—

THOMAS B. CHRISTIE, M.D., F.R.C.P.E.

Monday, 6*th May* 1872.

D. MILNE HOME, LL.D., Vice-President, in the Chair.

The following Communications were read:—

1. On the Chemical Efficiency of Sunlight. By James Dewar, Esq.

Of all the processes proposed to measure varying luminous intensities by means of chemical effects, not one has yet been expressed in strictly dynamical measure. This is owing to the very small amount of energy to be measured necessitating very peculiar processes for its recognition. The chemical actions generally induced by light are of the "Trigger" or "Relay" description; that is, bear no necessary relation to the power evolved by the transformation. There is one natural action of light continuously at work of a very different kind in the decomposition of carbonic acid by plants, necessitating a large absorption of energy, and thus enabling us to ascertain the proportion of the radiant power retained, through the chemical syntheses effected.

So far as I am aware, the following passage extracted from Helmholtz's Lectures "On the Conservation of Energy," delivered

at the Royal Institution in 1864, and published in the "Medical Times and Gazette," contains the first estimate of the chemical efficiency of sunlight. "Now, we have seen already, that by the life of plants great stores of energy are collected in the form of combustible matter, and that they are collected under the influence of solar light. I have shown you in the last lecture that some parts of solar light—the so called chemical rays, the blue and the violet which produce chemical action—are completely absorbed and taken away by the green leaves of plants; and we must suppose that these chemical rays afford that amount of energy which is necessary to decompose again the carbonic acid and water into its elements, to separate the oxygen, to give it back to the atmosphere, and to collect the carbon and hydrogen of the water and carbonic acid in the body of the plant itself. It is not yet possible to show that there exists an accurate equivalent proportion between the power or energy of the solar rays which are absorbed by the green leaves of plants, and the energy which is stored up in the form of chemical force in the interior of the plants. We are not yet able to make so accurate a measurement of both these stores of energy, as to be able to show that there is an equivalent proportion. We can only show that the amount of energy which the rays of the sun bring to the rank is completely sufficient to produce such an effect as this chemical effect going on in the plant. I will give you some figures in reference to this. It is found in a piece of cultivated land producing corn or trees, one may reckon per year and per square foot of land 0·036 lb. of carbon to be produced by vegetation. This is the amount of carbon, which during one year, on the surface of a square foot in our latitude, can be produced under the influence of solar rays. This quantity, when used as fuel and burnt to produce carbonic acid, gives so much heat that 291 lbs. of water could be heated 1° C. Now we know the whole quantity of solar light which comes down to one square foot of terrestrial surface during one second, or one minute, or one year. The whole amount which comes down during a year to one square foot is sufficient to raise the temperature of 430,000 lbs. of water 1° C. The amount of heat which can be produced by fuel growing upon one square foot during one year is, as you see from these figures, a very small fraction of the whole amount of solar

heat which can be produced by the solar rays. It is only the 1477th part of the whole energy of solar light. It is impossible to determine the quantity of solar heat so accurately that we could detect the loss of so small a fraction as is absorbed by plants and converted into other forms of energy. Therefore, at present, we can only show that the amount of solar heat is sufficient to produce the effects of vegetable life, but we cannot yet prove that this is a complete equivalent ratio." This estimate is, strictly speaking, the mean agricultural efficiency of a given area of land, cultivated as forest, and considering that active growth only takes place during five months in the year, we may safely adopt $\frac{1}{600}$th of the total energy of sunlight as a fair value of the conserved power, on a given area of the earth's surface in this latitude during the course of the summer. As chlorophyll in one or other of its forms is the substance through which light becomes absorbed, and chemical decomposition ensues, it would be interesting to acquire some idea of the storage of power, effected by a given area of leaf surface during the course of a day, and to compare this with the total available energy. Here we are dealing with strictly measurable quantities, provided we could determine the equation of chemical transformation.

Boussingault's recent observations on the amount of carbonic acid decomposed by a given area of green leaf seem to me to afford interesting data for a new determination of the efficiency of sunlight. In his experiments made between the months of January and October under the most favourable circumstances in atmospheres rich in CO_2 one square decimetre of leaf has decomposed in one hour, as a mean 5·28 cc of CO_2, and in darkness evolves in the same period of time 0·33 cc of CO_2. In other words, one square metre of green surface will decompose in twelve hours of the day, 6336 cc of CO_2, and produce in twelve hours of the night 396 cc of CO_2.

This quantity of carbonic acid decomposed does not represent the whole work of sunlight for the time, as water is simultaneously attacked in order to supply the hydrogen of the carbo-hydrates. Boussingault, in summing up the general results of his laborious researches on vegetable physiology, says, "Si l'on envisage la vie végétale dans son ensemble, on est convaincu que la feuille est la première étape des glucoses que, plus ou moins modifies, on trouve

répartis dans les diverses parties de l'organisme; que c'est la feuille quì les élabore aux depens de l'acid carbonique et de l'eau."—P. 415, Am. de Chemie, tom xiii. The fundamental chemical re-action taking place in the leaf, may therefore be represented as follows:—

$$(1) \quad CO,O \quad + \quad H_2O \quad = \quad CO,H_2 \quad + \quad O,O$$
$$(2) \quad 6(CO.H_2) \quad = \quad C_6H_{12}O_6$$

In the first equation carbonic acid and water are simultaneously attacked with the liberation of a volume of oxygen equal to that of the original carbonic, together with the formation of a substance having the composition of methylic aldelyde. The second equation represents the condensation of this aldelyde into grape sugar. The transformation induced in (1) necessitates the absorption of a large amount of energy; and if we neglect the heat evolved in the combination of nascent CO and H_2, which can be shown to be very little, the calculated result is made a maximum: whereas the con densation of (2) being attended with an evolution of heat, diminishes considerably the amount of power required. Happily Frankland's direct determination of the thermal value of grape sugar leaves no doubt as to the true equivalent of work done in its formation. Taking the following thermal value $CO,O = 68{,}000$, $H_2, O = 68{,}000$, $C_6H_{12}O_6 = 642{,}000$, 1c centimetre of CO_2 decomposed as in (1) would require 6·06 gramme units of heat, or its light equivalent; whereas the complete change into grape sugar of the same amount of carbonic acid requires only 4·78 gramme units. But we have seen before 1 square decimetre of green leaf functions at the rate of 5·28cc of carbonic acid assimilated per hour, therefore $(5{\cdot}28) \times (4{\cdot}78) = 25{\cdot}23$ represents the number of gramme heat units conserved through the absorption of light in the above period of time. Pouìllet estimates the mean total solar radiation per square decimetre exposed normally to the sun's rays in or near Paris per hour as 6000 gramme units, so that $6000 \div 25{\cdot}23 = \frac{1}{238}$ represents the fraction of the entire energy conserved. The estimate is by no means too little, as Boussingault has shown the leaf may function at twice the above rate for a limited time.

In connection with equation (1), above given, as representing the action of sunlight on the leaf, it is worthy of remark, that

supposing the carbonic acid and water equally efficient as absorbing agents of the vibratory energy (although each has a specific absorption for certain qualities of rays), then the decomposition of the two compound molecules may take place continuously side by side, owing to the equality of the thermal equivalents of carbonic oxide and hydrogen. We already know, from the laborious researches of Tyndall, how thoroughly aqueous vapour retains thermal radiations; and Janssen has further shown that the same substance has a strong absorptive action on the rays of light of low refrangibility (just those rays that are in part selected by chlorophyll), producing the well-known atmospheric lines of the solar spectrum. The presence, therefore, of varying quantities of aqueous vapour in the atmosphere in all probability produces a considerable difference of rate in the decomposition effected by the leaf, and may, in fact, end in carbonic acid and water being attacked in another ratio than that given as the fundamental equation of decomposition. Thus the same plant in different atmospheric conditions may elaborate different substances.

2. On the Rainfall of the Continents of the Globe. By Alexander Buchan, Secretary of the Scottish Meteorological Society.

This paper was illustrated by two large charts of the world showing, by ISOHYETAL LINES, the rainfall over the different continents in January and July; two large charts showing the months of least and greatest rainfall in Europe, north Africa, and west Asia; and by six sets of smaller charts of thirteen each, showing, by isohyetal lines, the monthly and annual rainfall of Europe, Asia, Australasia, North America, Africa, and parts of South America. The data laid down on these eighty-two charts were taken from a Table comprising about 2000 good averages of rainfall, calculated or collected by the author.

On comparing the results of the rainfall with the author's charts of Atmospheric Pressure and Prevailing Winds, published in the Society's Transactions,* the broad principles regulating aqueous precipitation are chiefly these:—

* Vol. xxv. p. 575, *et seq.*

1. When the prevailing wind has previously traversed a large extent of ocean, the rainfall is moderately large.

2. If the winds are at the same time advancing into colder regions, the rainfall is largely increased; and if a range of mountains lie across their onward path, the rainfall is also thereby largely increased on the side facing the prevailing winds, and reduced over the regions lying on the other side.

3. If the winds, though arriving from the ocean, have not traversed a considerable extent of it, the rainfall is not large.

4. If the winds, even though having traversed a considerable part of the ocean, yet on arriving at the land proceed into lower latitudes, or regions markedly warmer, the rainfall is small or *nil.*

3. On the Lunar Diurnal Variation of Magnetic Declination at Trevandrum, near the Magnetic Equator. By J. A. Broun, F.R.S.

The author gives the results derived from different discussions of nearly eighty thousand observations, made hourly during the eleven years 1854 to 1864. They are as follows:—

1. That the lunar diurnal variation consists of a double maximum and minimum in each month of the year.

2. That in December and January the *maxima* occur near the times of the moon's upper and lower passages of the meridian; while in June and July they occur six hours later, the *minima* then occurring near the times of the two passages.

3. The change of the law for December and January to that for June and July does not happen, as in the case of the solar diurnal variations, by leaps in the course of a month (those of March and October), but more or less gradually for the different maxima and minima.

4. While the lunar diurnal variation changes the hours of maxima and minima more gradually than the solar diurnal variation, it also makes the greatest change at different times; thus the solar diurnal variation changes completely during the month of March, or from February to April, while the lunar diurnal variation makes the greatest change from April to May. The second

great change which happens for the sun, between September and November, occurs earlier, or between September and October for the moon.

5. The range of the variation is greatest in January, and is least in May and October; the arc, including the mean diurnal variation for January, from eleven years' observations, being nearly 0′·5, while in the latter months the ranges were nearly 0′·18 and 0′·14 respectively; the range for July being 0′·26.

The author states, that, in a paper already published,* he has shown that the range of the diurnal variation amounts sometimes to five minutes (5′·0), which, from the less value of the horizontal force, would be equivalent to about twelve minutes (12′·0) in England; and that the diminution of range appearing in the mean of many lunations is due to the combination of variations following different laws.

6. The ranges of the mean lunar and mean solar diurnal variations thus obey different laws with reference to the period of the year; the range of the former in January being nearly double that in any month from May to September, while the range of the latter in August is nearly double that in January.

In the discussion for the change of the law which might be due to the moon's passing from one hemisphere to the other, the author found different results for different months of the year; this led him to perform the calculations in a new way, described by him, in which the law derived from observations made during the day is separated from that obtained from observations made during the night. From this discussion it follows—

7. That the action of the moon on the declination needle is, in every month of the year, greater during the day than during the night; the range of the oscillation in January and June being nearly four times greater during the day than during the night, the ratio being less in the intermediate months.

When the results are derived from the forenoon hours only, or from the afternoon hours only, the range in January is six times greater than that derived from the night hours only.

It also appears that the law derived from the night hours varies little in the course of the year; it is only that derived from the

* Trans. Roy. Soc., Edin. vol. xxiv. p. 673

day hours which becomes inverted in passing from January to July. It follows—

8. That the principal, if not the only, cause of change in the amount of the lunar action at Trevandrum, near the magnetic equator, for the moon on different meridians, depends on whether the sun is shining on the place of the needle or not.

The author finds—

9. That the area of the curve representing the lunar diurnal variation in the mean of the group of months, October to April, for the half orbit about Perigee, is to that for the other half orbit as 1·18 : 1; while for the group of months, May to September, the ratio is 1·31 : 1; the moon's action appearing to diminish more rapidly with the distance from the earth, when both moon and earth are farthest from the sun. As the mean distances of the moon from the earth in the two half orbits are nearly as 1 to 1·07, it appears that the mean range for Perigee and for Apogee, derived from both groups, varies nearly as the inverse cube of the distance, as in the case of the tides.

Monday, 20th May 1872.

PROFESSOR SIR ROBERT CHRISTISON, Bart., President, in the Chair.

The following Communications were read:—

1. Some Helps to the Study of Scoto-Celtic Philology, by the Hon. Lord Neaves.

(*Abstract.*)

Lord Neaves read a paper entitled "Some Helps to the Study of Scoto-Celtic Philology," in which, after noticing the mistaken tendencies of the Celtic scholars of former times, both Irish and Scotch, as to the origin and affinities of Gaelic, and adverting to the fact now firmly fixed that it was an Aryan or Indo-Germanic tongue, he submitted a statement of some of the imitations or disguises which words underwent or assumed in passing into Gaelic. Thus it was a peculiarity of Gaelic to avoid the letter *p*, which it

did in various ways. Sometimes it dropped that letter, as when it changed the Latin *Pater* into *Athir*, the Latin *piscis* into *iasg*, *plenus* into *làn*, &c. Sometimes it changed the *p* into a gutteral c, g, or ch, as *seachd* for *septem*, *feasgar* for *vesper*. It did this even in borrowed words, as when the Church term *Pasch* for Easter was changed into *Caisg;* the Latin *purpur* into *corcur*. It was another peculiarity of Gaelic to omit the letter *n* before certain other consonants, so that *centum* became *cead*, *quinque* became *coig*, *mensis*, *mios ; infernum*, *ifrinn ; inter*, *eadar*. The Latin *v* or English *w* was generally represented in Gaelic at the beginning of words by *f:* thus *vir*, *fear ; verus*, *fior ; vinum*, *fion ; vates*, *faidh ;* &c. The old Irish word for a widow was *fedb*. Two remarkable prefixes occurring frequently in Gaelic, *do* and *so*, correspond to similar prefixes *du* and *su* in Sanscrit : *do* and *du* meaning "evil or difficulty," and *so* and *su* meaning "goodness or facility." These prefixes are very abundant in those two languages at the two extremes of the Aryan field, but though represented also in Greek, are scarcely or very slightly perceptible in the intermediate tongues.

An attention to these and other changes which words undergo in passing into Gaelic would greatly facilitate the study of this remarkable tongue, which it is not creditable to Scotchmen to neglect as they have done. The comparative forms of the inflections of words also deserve attention, and on this subject reference might be made to an interesting lecture on the Gaelic, by Professor Geddes of Aberdeen.

2. Some Observations on the Dentition of the Narwhal (*Monodon monoceros*). By Professor Turner.

The author expressed his concurrence with those anatomists who hold that the two tusks of the narwhal are situated in sockets in the superior maxillary bones, and not, as was stated by the Cuviers, in the premaxillæ, or partly in the pre- and partly in the superior maxillæ. He then proceeded to relate some further observations on the dentition of the narwhal, and pointed out, both in the skull of a young male and in those of three well grown fœtuses, an elongated canal on each side of the upper jaw, parallel and inferior to the tusk socket, which had the appearance of a socket

for a supplementary tooth, although none protruded from it. In the young male a minute denticle was seen at the bottom of this socket.

He then described a dissection he had made of the upper jaw of a male fœtus, 7¼ inches long, given him by Mr C. W. Peach, in which, imbedded in the gum on each side, were two well-formed dental papillæ, barely visible to the naked eye. Each papilla was contained in a well-defined tooth sac. Calcification of the papillæ or of the wall of the tooth sac had not commenced. The minute structure of these embryonic teeth was next described. The more anterior of the two papillæ was $\frac{2}{10}$ths inch behind the tip of the jaw, and the more posterior lay about $\frac{1}{10}$th inch behind the anterior.

No rudimentary teeth were found in the lower jaw.

The formation of bone had only just begun in the fibrous matrix of the maxillary bones; but in the lower jaw a very decided ossification of the fibrous membrane investing the cartilage of Meckel had commenced.

3. On the occurrence of *Ziphius cavirostris* in the Shetland Seas, and a comparison of its Skull with that of Sowerby's Whale (*Mesoplodon Sowerbyi*). By Professor Turner.

This paper contained a brief historical sketch of *Ziphius cavirostris.* The skull of a specimen caught at sea in 1870, off Hamna Voe, Northmaven, Shetland, was then described, and this skull was compared with previously recorded specimens. A brief historical sketch of Sowerby's whale was then given, a skull in the Edinburgh Museum of Science and Art was described, and reasons were advanced for associating it with the genus *Mesoplodon* rather than with *Ziphius.*

4. On the Maternal Sinus Vascular System of the Human Placenta. By Professor Turner.

The author gave a brief sketch of the various theories which have been advanced by Velpeau, R. Lee, Braxton Hicks, the Hunters, Owen, Weber, J. Reid, J. Goodsir, Virchow, Kölliker, Van

Der Kolk, Arthur Farre, and Ercolani regarding to the relations of the maternal blood-vessels to the placenta and chorionic villi. He then proceeded to state the results of his own observations on various specimens of placentæ, some of which had been separated at the full time, others prematurely, and on three specimens attached to the uterine wall. Two of these latter were from women at or about the full period of gestation, whilst the third was from a woman who died undelivered in the sixth month of pregnancy. In one of the attached specimens a pipe had been introduced into a uterine vein in the broad ligament, and a coloured gelatine injection had been passed along the venous sinuses in the muscular wall, and the utero-placental veins into the placenta. The utero-placental veins were followed through the decidua serotina, and were seen to pierce the uterine surface of the placenta. The walls of these veins were so delicate that they tore through on the application of very slight force. Thin sections made through the placenta and the adjacent part of the uterine wall permitted the author to trace a direct continuity of the injection within the placenta with that within the utero-placental veins and uterine sinuses, and showed the one to be continuous with the other. The injection also passed into veins of considerable size, situated within the decidua reflexa, near the attached border of the placenta. In another attached specimen, the intra-placental sinus system was injected with coloured gelatine from a pipe inserted into one of the uterine arteries, and the injection of the system of intercommunicating spaces within the placenta was as readily made as in the specimen where the injection was passed through the uterine vein. In the third attached specimen, the injecting pipe was introduced into the cut face of a section through the placenta itself, and the intra-placental sinus system was not only distended, but some of the injection had even entered the utero-placental veins.

Thin sections of the injected placentæ had been made and examined both with low and high powers of the microscope. Drawings, greatly enlarged, of the appearances seen on examining these sections were shown to the Society, and the author pointed out that these were to be regarded as actual representations of the objects, and not, as had previously been almost universally the case, mere diagrammatic conceptions of what the anatomist might consider to

be the character of the arrangement. The chorionic villi were seen in these sections to be cut across longitudinally, obliquely, and transversely, and the villi were not in contact with each other by their surfaces, but separated by intermediate freely-communicating spaces, filled with coloured gelatine. These spaces constituted the intra-placental maternal sinus vascular system. Thin sections examined with high powers showed multitudes of red-blood corpuscles lying in the coloured gelatine, which corpuscles had undoubtedly been in these sinuses before the injection had been passed into them, and from their position were the corpuscles of the maternal blood. The ready manner in which the injection flowed into the intra-placental sinuses, either when passed directly into the placenta, or through the artery, or through the vein, the regularity and uniformity of the pattern produced by the injection when set, and the abundance of blood corpuscles present in the sinuses, mingled with the injection, seemed to the author to substantiate the view that these sinuses are a natural system of intercommunicating spaces for the transmission of the maternal blood through the interior of the placenta; and not as some have maintained, artificially produced by the extravasation of injection from the uterine vessels into the placenta.

The author then proceeded to describe the structure of the chorionic villi, to show their relations to the decidua serotina and the decidual bars which pass into the interior of the placenta, and to discuss the views which have been advanced, whether the villi hang naked in the maternal blood, or whether they are invested either by a prolongation of the lining membrane of the maternal blood-vessels, or by the cells of the decidua, or by both.

The following Gentleman was admitted a Fellow of the Society:—

Rev. Hugh Macmillan, LL.D.

Monday, 3d June 1872.

PROFESSOR W. J. MACQUORN RANKINE, Vice-President, in the Chair.

The following Communications were read :—

1. On Dimorphic Flowers of *Cephaelis Ipecacuanha*, the Ipecacuan Plant. By Professor Balfour.

I have reported already to the Society (p. 688) the results of the cultivation of the Ipecacuan plant in the Botanic Garden, and its successful propagation by Mr M'Nab by root-cutting. By this means it has been sent in considerable quantity to Calcutta, under the direction of the Secretary of State for India. From the Garden at Kew, in 1863, a plant was sent out to Dr King, and of late he has been successful in propagating it by cuttings of the stem above ground. So that from both sources there seems to be every prospect of the plant being extensively cultivated in India, the climate of which in many places is favourable for its growth. The so-called root of the Ipecacuan may be said to be composed of a sort of underground stem capable of producing leaf-buds, as well as true roots.

I have already stated that the plants in the Botanic Garden have been derived from two sources,—one from a plant sent by Sir Wm. Hooker more than 40 years ago, and which he had procured from Mr M'Koy of Liege; the other is from plants sent from Rio Janeiro by Dr Gunning. There is an apparent difference in the characters of the plants from these two sources, but not such as to amount to a specific distinction. Hooker's plant has flowered pretty freely, but never produced fruit until last year, when the pollen was artificially applied from one flower to another. All the plants from this source have long stamens and short styles.

The plants sent by Dr Gunning have grown well, but it is only recently that they have flowered, and now there are several specimens in flower, and some are fruiting after artificial impregnation. In this series of plants there are evident dimorphic flowers. In some the stamens are long and the style is short; while in others the style is long, projecting much beyond the corolla, while the stamens are short.

It would appear that successful fertilisation may be effected by applying the pollen from the long stamens to the stigma of the long styles.

The partial fruiting which took place in the heads of flowers in the Hookerian plants may have depended on the fact that there were only produced flowers with long stamens and short styles, and although when pollen was applied from one flower to another fertilisation was effected, still it was by no means fully successful, only two or three of the flowers in the head producing fruit. The flowers are sweet-scented with a delicate odour.

One of the largest plants has the following dimensions :—

Height of plant, ...	$12\frac{1}{2}$ inches.
Length of leaves, . . .	5 „
Breadth of leaves, . . .	2 „
Peduncle (length), . . .	1 inch
Greatest circumference of stem, .	$\frac{1}{2}$ „

2. On the Crinoids of the "Porcupine" Deep-Sea Dredging Expedition. By Professor Wyville Thomson.

Seven species belonging to the Echinoderm order CRINOIDEA, were procured during the "Porcupine" dredging expeditions of 1869 and 70. Four of these belong to the free section of the order, and are referred to the genus ANTEDON.

1. *A. escrichtii*, J. Müller.

This fine species is abundant off the coast of Greenland, but so far as I am aware, it does not occur in the seas of Scandinavia. Several hauls of the dredge in the cold area in the channel between Scotland and Faeroe, yielded many examples, the largest of which, however, fell somewhat short of the dimensions of the largest specimens from Greenland. *Antedon escrichtii* was associated in the Faeroe channel with *Ctenodiscus crispatus*, an Asteridean which had been met with previously only in the Greenland seas. A single example of a pentacrinoid in an early stage was found associated with *Antedon escrichtii*. It resembled closely the larva of *Antedon sarsii*, but the specimen was not sufficiently perfect for a critical examination.

2. *A. sarsii*, Duben and Koren.

More or less complete specimens or fragments of this widely distributed species came up in nearly every one of the deep hauls of the dredge, from the Faeroe Islands to Gibraltar. One or two small examples of the pentacrinoid were procured in the Faeroe Channel.

3. *A. rosaceus*, Linck.

Frequent in water of moderate depth. Many examples of the form known to continental naturalists under the name of *A. mediterraneus*, Lam. sp., were dredged in the Mediterranean off the coast of Africa. I do not feel satisfied that this is identical with *Antedon rosaceus* of the coast of Britain, although the two specific names are usually regarded as synonyms. There is a great difference between them in habit; a difference which it is difficult to define.

4. *A. celticus*, Barrett.

This species, which is at once distinguished by the extreme length of the dorsal cirri, is abundant at depths of 40 to 60 fathoms in the Minch, and we also met with it in local patches to 150 fathoms off the north coast of Scotland.

The remaining three Crinoids belong to the section of the Order which are permanently stalked. Two of the three are new to science, and the third was discovered in the year 1864 by G. O. Sars, in the deep water off the Loffoden Islands.

Up to the present time two recent species have been described belonging to the Family Pentacrinidæ. Both of these were known only from the deep water of the seas of the Antilles. Since the discovery of the first of these in the year 1755, they have been regarded with special interest, both on account of their great beauty, and of the singular relation which they bear to some of the most abundant and characteristic fossils of the palæozoic and mezozoic formations.

Pentacrinus asteria, L., the species first described by Guettard, and afterwards very carefully worked out by Johannes Müller, has a stem sometimes nearly a metre in length consisting of a multitude of discoidal joints about every seventeenth of which bears a circle of five long cirri which spread out rigidly and abruptly

from the joint, turning down hooklike towards the tips. Each cirrus consists of about 36 joints. The nodal joint, that is to say the joint modified for the insertion of the cirri, is single; but it is united to the joint beneath by a peculiar suture with much of the character of a syzygy. Most of the examples of *P. asteria* which have reached Europe have had the stem recently broken. In one however in my possession, the stem, which is unusually short, had evidently given way at one of these joints long before the death of the animal, for the surface of the terminal joint is smoothed and rounded, and the terminal row of cirri are curved over it. This example, at all events, must have lived for some time free.

In *Pentacrinus asteria*, the basal plates of the cup project like small round buttons over the ends of the salient angles of the first stem joint. The first radials are connected with the second radials by a true joint with muscles and ligaments, and the second radial is united to the radial axillary by a syzygy. There are from 70 to 120 pinnated arms. There is constantly a syzygy on each branch at the first joint beyond each bifurcation, but there are few syzygies on the arms after their last bifurcation, although in some specimens one is met with here and there.

All the examples of *P. asteria* in European museums have lost the soft parts and the disk; but I have one example which is complete. The mouth is central, and five radial grooves pass from the edge of the mouth-opening to the proximal ends of the arms, and become continuous with the brachial grooves, dividing with each bifurcation. The perisom of the disk is covered with irregular calcareous plates, and at the free inner angles of the interradial spaces these plates become closer, and form a solid kind of boss; but there are no distinct oral plates. A rather long anal tube occupies the centre of one of the interradial spaces.

Pentacrinus mülleri, Œrstedt, seems to be more common than *P. asteria* especially off the Danish West Indian Islands. The whole animal is more delicate in form. The stem attains nearly the same height, but is more slender. The nodes occur about every twelfth joint and at every node two stem-joints are modified. The upper joint bears the facets for the insertion of the cirri, and the second is grooved to receive the thick basal portions of the cirri, which bend downwards for a little way closely adpressed to the

stem before becoming free. The cirri are much shorter than in *P. asteria*. The syzygy is between the two modified joints. In all complete specimens which I have seen, the stem has evidently been separated for long at one of these syzygies. I described some years ago a specimen in which this was the case, and suggested that in that instance the animal had lived for some time free. I have since seen several other examples in the same condition, and I believe that the disengagement at a certain stage of growth is habitual. The arrangement of the joints and syzygies in the cup is the same in *P. mülleri* as in *P. asteria*, only the syzygy between the second radial and the radial axillary is not so complete. The arms are more delicate, and appear never to exceed thirty in number. The number of syzygies is very variable; sometimes they are confined, as in *P. asteria*, to the first joint after a bifurcation, and sometimes they occur at intervals all along the arms. The structure of the disk is the same as in *P. asteria*, but its texture is more delicate, and the calcareous pieces are smaller and more distant.

On the 21st of July 1870, Mr Gwyn Jeffreys, dredging from the "Porcupine," at a depth of 1095 fathoms, latitude 39° 42′ N. long. 9° 43′ W., with a bottom temperature of 4°·3 C., took about twenty specimens of a handsome PENTACRINUS involved in the hempen tangles attached to the dredge.

1. *P. wyville-thomsoni*, Jeffreys.

This species is intermediate in some of its characters between *P. asteria* and *P. mülleri*, it approaches the latter however most nearly. In a mature specimen the stem is about 120 mm. in length and consists of five to six internodes. The whorls of cirri towards the lower part of the stem are 40 mm. apart, and the internodes consist of from thirty to thirty-five joints. The cirri are rather short, and stand out straight from the nodal joint or curve slightly downwards. There are usually eighteen joints in the cirri, the last forming a sharp claw. As in *P. asteria* the nodal joint is single, and a syzygy separates it from the joint immediately beneath it which does not differ materially in form from the ordinary internodal stem-joints. All the stems of mature examples of this species end inferiorly in a nodal joint surrounded by its whorl of cirri, which curve downwards into a

kind of grappling root. The lower surface of the terminal joint is in all smoothed and rounded, evidently by absorption, showing that the animal has long been free. This character I have already noted as occurring in some specimens of *P. mülleri* and in one at least of *P. asteria.* I have no doubt whatever that it is constant in the present species, and that the animal lives loosely rooted in the soft mud, and may change its place at pleasure by swimming with its pinnated arms: that it is, in fact, intermediate in this respect between the free species of *Antedon* and the permanently rooted fossil crinoids.

A young specimen of *P. wyville-thomsoni* gives the mode in which this freedom is acquired. The total length of this specimen is 95 mm., of which the head occupies 35 mm. The stem is broken off in the middle of the eighth internode from the head. The lowest complete internode consists of 14 joints, the next of 18, the next of 20, and the next of 26 joints. There are 8 joints in the cirri of the lowest whorl, 10 in those of the second; 12 in those of the third, and 14 in those of the fourth. This is the reverse of the condition in adult specimens, in all of which the numbers of joints in the internodes, and of joints in the cirri, decrease regularly from below upwards. The broken internode in the young example and the three internodes above it are atrophied and undeveloped; and suddenly at the third node from the head the stem increases in thickness and looks as if it were fully nourished. There can be no doubt that in early life the Crinoid is attached, and that it becomes disengaged by the withering of the lower part of the stem.

The structure of the cup is the same as in *P. asteria* and *P. mülleri.* The basals appear in the form of shield-like projections crowning the salient angles of the stem. Alternating with these we have well-developed first radials forming a closed ring and articulating to free second radials by muscular joints. The second radials are united by a syzygy to the radial axillaries, which as usual give off each two first brachials from their bevelled sides. A second brachial is united by syzygy to the first, and normally this second brachial is an axillary, and gives off two simple arms; sometimes, however, the radial axillary originates a simple arm only from one or both of its sides, thus reducing the

total number of the arms, and sometimes one of the four arms given off from the brachial axillaries again divides, in which case the total number of arms is increased. The structure of the disk is much the same as in the species of the genus previously known.

The APIOCRINIDÆ to which the remaining two fixed Crinoids must be referred, differ from all other sections of the order in the structure of the upper part of the stem. At a certain point considerably below the crown of arms the joints of the stem widen by the greater development of the calcified ring, the central cavity scarcely increasing in width. The widening of the stem-joint increases upwards until a pyriform body is produced, usually very elegant in form, in which one would suppose looking at the outside that the viscera were lodged. It is, however, nothing more than a symmetrical thickening of the stem, and the body cavity occupies a shallow depression in the top of it inclosed within the plates of the cup; the basals and radials are much thicker and more fully calcified than in other crinoids, but they are normally arranged.

The stem is usually long and simple, until near the base, where it forms some means of attachment; either as in the celebrated pear encrinites of the forest-marble, a complicated arrangement of concentric layers of cement which fix it firmly to some foreign body; or as in the chalk *Bourguetticrinus* and in the recent *Rhizocrinus*, an irregular series of jointed branching cirri.

The APIOCRINIDÆ attained their maximum during the Jurassic period, where they are represented by numerous and fine species of the genera *Apiocrinus* and *Millericrinus*. The chalk genus *Bourguetticrinus* shows many symptoms of degeneracy. The head is small, and the arms are small and short. The arm joints are so minute that it is difficult to make up anything like a complete series from the separate fragments scattered through the chalk in the neighbourhood of a cluster of heads. The stem, on the other hand, is disproportionately large and long, and one is led to suspect that the animal was nourished chiefly by the general surface absorption of organic matter, and that the head and special assimilative organs are principally concerned in the function of reproduction. The genus RHIZOCRINUS possesses all the essential characters of the family.

1. *R. lofotensis*, M. Sars.

This species was discovered in the year 1864, at a depth of about 300 fathoms, off the Loffoden Islands, by G. O. Sars, a son of the celebrated Professor of Natural History in the Uuiversity of Christiania; and it was described in detail by the latter in the year 1868. It is evidently a form of the Apiocrinidæ still more degraded than *Bourguetticrinus*, which it closely resembles. The stem is long and of considerable thickness in proportion to the size of the head. The joints of the stem are individually long and dice-box shaped, and between the joints spaces are left on either side of the stem alternately, as in *Bourguetticrinus*, and in the pentacrinoid of *Antedon* for the insertion of fascicles of contractile fibres. Towards the base of the stem branches spring from the upper part of the joints; and these, each composed of a succession of gradually diminishing joints, divide and re-divide into a bunch of fibres which expand at the ends into thin calcareous laminæ, clinging to small pieces of shell, grains of sand—anything which may improve the anchorage of the crinoid in the soft mud which is nearly universal at great depths.

In *Rhizocrinus* the basal series of plates of the cup are not distinguishable. They are masked in a closed ring at the top of the stem, and whether the ring be composed of the fused basals alone, or of an upper stem-joint with the basals within it forming a "rosette" as in the calyx of *Antedon*, is a question which can only be solved by a careful tracing of successive stages of development. The first radials are likewise fused, and form the upper wider portion of the funnel-shaped calyx The first radials are deeply excavated above for the insertion of the muscles and ligaments which unite them to the second radials by a true (or moveable) joint. One of the most remarkable points in connection with this species is, that the first radials, the first joints of the arm, are variable in number, some examples having four rays, some five, some six, and a very small number seven in the following proportions. Out of seventy-five specimens examined by Sars, there were—

15	with	4 arms.
43	"	5 "
15	"	6 "
2	"	7 "

This variability in so important a character, particularly when associated with so great a preponderance in bulk of the vegetative over the more specially animal parts of the organism, must undoubtedly be accepted as indicating a deterioration from the symmetry and compactness of the Apiocrinidæ of the Jurassic period.

The anchylosed ring of first radials is succeeded by a tier of free second radials, which are united by a straight syzygial suture to the next series—the radial axillaries. The surface of the funnel-shaped dilation of the stem, headed by the ring of first radials, is smooth and uniform, and the second radials and radial axillaries present a smooth regularly arched outer surface. The radial axillaries differ from the corresponding joints in most other known crinoids in contracting slightly above, presenting only one articulating facet, and giving origin to a single arm. The arms, which in the larger specimens are from 10 to 12 mm. in length, consist of a series of from about twenty-eight to thirty-four joints, uniformly transversely arched externally, and deeply grooved within to receive the soft parts. Each alternate joint bears a pinnule alternating on either side of the axis of the arm, and the joint which does not bear a pinnule is united to the pinnule-bearing joint above it by a syzygy: thus joints with muscular connections and syzygies alternate throughout the whole length of the arm.

The pinnules, twelve to fourteen in number, consist of a uniform series of minute joints united by muscular connections. The grooves of the arm and of the pinnules are bordered by a double series of delicate round fenestrated calcareous plates, which, when the animal is contracted and at rest, form a closely imbricated covering to the nerve and the radial vessel with its delicate cæcal tentacles. The mouth is placed in the centre of the disk, and radial canals, equal in number to the number of arms, pass across the disk, and are continuous with the arm grooves. The mouth is surrounded by a row of flexible cirri arranged nearly as in the pentacrinoid of *Antedon*, and is provided with five oval calcareous valve-like plates occupying the interradial angles, and closing over the mouth at will. A low papilla in one of the interradial species indicates the position of the minute excretory orifice.

Rhizocrinus lofotensis is a very interesting addition to the British

Fauna. We met with it in the Faeroe Channel in the year 1869,—three examples, greatly mutilated, at a depth of 530 feet, with a bottom temperature of 6°·4 C. Station 12 (1868) —Several occurred attached to the beards of *Holteniæ* off the Butt of the Lews, and specimens of considerably greater size were dredged in 862 fathoms off Cape Clear. The range of this species is evidently very wide. It has been dredged by G. O. Sars off the north of Norway; by Count Pourtales, in the Gulf-stream off the coast of Florida; by the naturalists on board the "Josephine" on the "Josephine Bank" near the entrance of the Strait of Gibralter; and by ourselves between Shetland and Faeroe, and off Ushant and Cape Clear.

The Genus Bathycrinus (n. g.) must also apparently be referred to the Apiocrinidæ, since the lower portion of the head consists of a gradually expanding funnel-shaped piece, which seems to be composed of coalesced upper stem-joints.

1. *B. gracilis* (n. sp.).

The stem is long and delicate, in one example of a stem alone, which came up in the same haul with the one perfect example which was procured, it was 90 mm. in length. The joints are dice-box shaped as in *Rhizocrinus*, long and delicate, towards the lower part of the stem 3·0 mm. in length by 0·5 mm. in width in the centre, the ends expanding to a width of 1·0 mm. As in *Rhizocrinus*, the joints of the stem diminish in length towards the head, and additions are made in the form of calcareous laminæ beneath the coalesced joints which form the base of the cup.

The first radials are five in number. They are closely opposed, but they do not seem to be fused as in *Rhizocrinus*, as the sutures show quite distinctly. The centre of each of the first radials rises into a sharp keel, while the sides are slightly depressed towards the sutures, which gives the calyx a fluted appearance, like a folded filter paper. The second radials are long and free from one another, joining the radial axillaries by a straight syzygial union. They are most peculiar in form. A strong plate-like keel runs down the centre of the outer surface, and the joint is deeply excavated on either side, rising again slightly towards the edges. The radial axillary shows a continuation of the same keel through its lower half, and midway up the joint the

keel bifurcates, leaving a very characteristic diamond-shaped space in the centre towards the top of the joint. Two facets are thus formed for the insertion of two first radials. The number of arms is therefore ten. The arms are perfectly simple, and in our single specimen consist of twelve joints each. There is no trace of pinnules, and the arms resemble in character the pinnules of *Rhizocrinus.* The first brachial is united to the second by a syzygial joint, but after that the syzygies are not repeated, so that there is only one of these peculiar junctions in each arm. The arm-grooves are bordered by circular fenestrated plates as in *Rhizocrinus.*

Certain marked resemblances in the structure of the stem, in the structure of the base of the cup, and in the form and arrangement of the ultimate parts of the arms, evidently associate *Bathycrinus* with *Rhizocrinus;* but the differences are very wide. Five free keeled and sculptured first radials replace the uniform smooth ring formed by these plates in *Rhizocrinus.* The radial axillaries give off each two arms, thus recurring to the more usual arrangement in the order, and the alternate syzygies on the arms, which form so remarkable a character in *Rhizocrinus*, are absent.

Only one nearly complete specimen and a detached stem of this very remarkable species were met with, and they were both brought up from the very greatest depth which has as yet been reached with the dredge, 2435 fathoms, at the mouth of the Bay of Biscay, 200 miles south of Cape Clear.

3. Laboratory Notes. By Professor Tait.

1. On Thermo-electricity: Circuits with more than one Neutral Point. (With a Plate.)

Having lately obtained from Messrs Johnson & Matthey some wires of platinum, and of alloys of platinum and iridium, I formed them into circuits with iron wire of commerce; and noticed that with all, excepting what is called "soft" platinum, there is more than one neutral point situated below the temperature of low white heat, and that at higher temperatures other neutral points occur. This observation is, in itself, highly interesting; but my first impression was one of disappointment, as I imagined it depended on some peculiarity of the platinum metals, which I had hoped would

furnish me with the means of accurately measuring high temperatures (by a process described in previous notes of this series). As this hope may possibly not be realised, I can as yet make only rough approximations to an estimation of the temperatures of these neutral points.

So far as I am aware, the phenomenon discovered by Cumming and analysed by Thomson has hitherto been described thus: When the temperature of the cold junction is below the neutral point, the gradual raising of the temperature of the other produces a current which increases in intensity till the neutral point is reached, thenceforth diminishes; vanishes when one junction is about as much above the neutral point as the other is below it, and is *reversed* with gradually increasing intensity as the hot junction is farther heated. To discover how my recent observation affects this statement, I first simply heated one junction of a circuit of iron and (hard) platinum gradually to whiteness, by means of a blowpipe, and observed the indications of a galvanometer—both during the heating and during the subsequent cooling when the flame was withdrawn. The heating could obviously not be effected at all so uniformly as the cooling; but, making allowance for this, the effects occurred in the opposite order, and very nearly at the same points of the scale in the descent and in the ascent. [I have noticed a gradual displacement of the neutral points when the junction was heated and cooled several times in rapid succession; but as my galvanometer, though it comes very quickly to rest, is not quite a *dead-beat* instrument, I shall not farther advert to this point till I have made experiments with an instrument of this more perfect kind, which is now being constructed for me.] The observed effect of heating, then, was a rise from zero to 110 scale divisions when the higher temperature was that of the first neutral point, then descent to 95 at a second neutral point, then ascent to a third, descent to a fourth, neither of which could be at all accurately observed, and finally ascent until the junction was fused.

With an alloy of 15 per cent. iridium and 85 per cent. platinum, the galvanometer rose to 53·5 at a neutral point, then fell to − 50 at a second, then rose to a third at − 39·5, and thence fell, but I could not observe a possible fourth neutral point on account of the

fusion of the iron. As shown on the plate, the first of these occurs at about 240° C. of a mercurial thermometer.

With another alloy supposed to be of the same metals, but of which I do not yet know the composition, also made into a junction with iron, the behaviour was nearly the same, but the readings at the successive neutral points were 28, – 137, – 132. The temperature of the first is about 200° C. by mercurial thermometer.

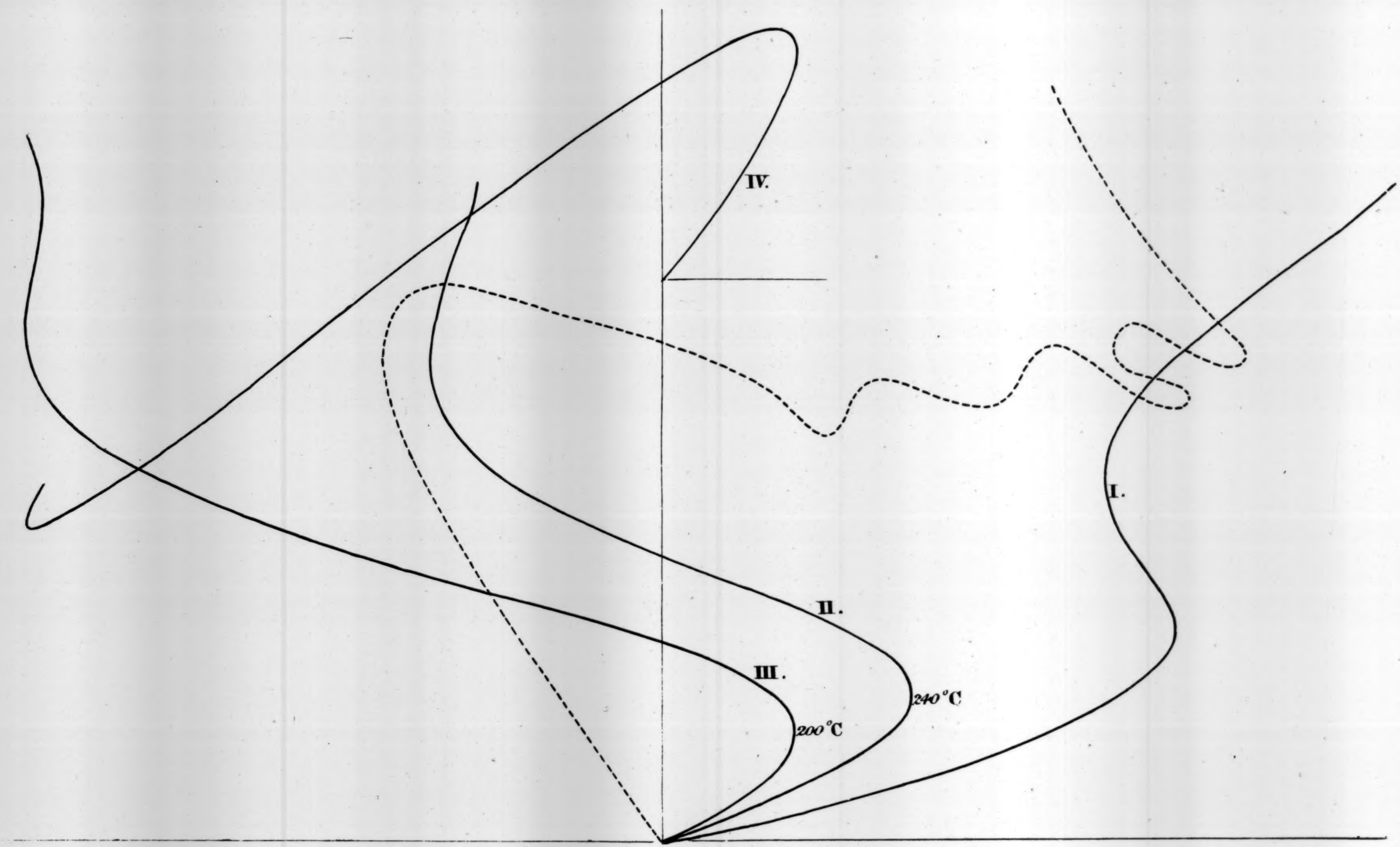
An iron-palladium circuit showed no neutral points within the great range of temperatures mentioned above; though it showed a remarkable peculiarity which must be more closely studied, as it appears to point to the cause of the above effects in a property of iron. It was therefore employed to give (very roughly) an indication of the actual temperatures in these experiments. But as for this purpose it is necessary to measure the simultaneous indications of two circuits whose hot and whose cold junctions are respectively at the same temperatures, I was obliged to employ a steadier source of heat than the naked flame. I therefore immersed the hot junctions in an iron crucible containing borax glass, subsequently exchanged for a mixture of fused carbonate of soda and carbonate of potash; but, to my surprise, the former of these substances at a red heat disintegrated both the platinum and the alloy, and thus broke both circuits without sensibly acting on the iron, while the mixture (evidently by the powerful currents discovered by Andrews, *Phil. Mag.* 1837) interfered greatly with the indications of the thermo-electric circuit, as will be seen by the dotted curve in the plate. [I may remark here that the deviations of this curve from its form when these currents are prevented are quite easily observed and plotted by the process next to be mentioned, so that the study of the Andrews' effect may be carried out with great accuracy by my method.] Finally, determining to dispense altogether with fused salts, which conduct too well besides acting on the metals, I simply suspended a red-hot bombshell, vent downwards, in such a way that the hot junction was near its centre. This arrangement worked admirably, until a white heat was required, for this melted the shell. In its place a wrought iron tube (an inch in bore, four inches long, half an inch thick, and closed at the upper end) has been substituted and answers excellently. It does not cool too fast for accurate reading at the higher temperatures, and by elevating

it by degrees from over the hot junction we can make the cooling fast enough at the lower ranges. In fact, I believe that if I do not succeed in getting a sufficient number of practically infusible metals to construct my proposed thermometric arrangement, I may be able to make a fair approximation to temperatures by simple time observations made with the hot tube, surrounded by some very bad conductor, such as sand, where the surface in contact with the air is always comparatively cool, and where therefore we can accurately calculate the rate of cooling.

Curves I., II., III., in the plate were drawn by means of this apparatus. The hot junction consisted of an iron wire, a palladium wire, and (for the several curves in order)—I. Hard platinum; II. Pt 85, Ir 15; III. The other alloy of Pt and Ir. The free ends of the palladium wire, and of the platinum or alloy, were joined to iron wires, and the junctions immersed in test-tubes filled with water resting side by side in a large vessel of cold water. The other ends of these three iron wires, and the wires of the galvanometer, were led to a sort of switch, by means of which either circuit could be instantly made to include the galvanometer. Readings were taken of each circuit as fast after one another as possible (with the galvanometer I employed about 6·5 seconds was the *necessary* interval), and the mean of two successive readings of one circuit was taken as being at the same temperature as that of the intermediate reading of the other.

The indications of these curves are very curious as regards the effect of even small impurities on the thermo-electric relations of some metals. It is probable, from analogy, that the curve for iron and *pure* platinum, in terms of temperature, would be (approximately, at least; even if it should be the iron, and not the platinum metal, which is represented by a broken or curved line) a parabola with a very distant vertex. And it appears probable that when the wire of curve III. is analysed it will be found to contain even a larger percentage of iridium (?) than that of curve II.

I find, by tracing these curves on ground glass, allowing for the difference between temperatures and the indications of an Fe-Pd circuit, and superposing them on a nest of parabolas with a common vertex and axis, that they can be closely represented by successive portions of different parabolas (with parallel axes) whose


IV.
I.
II.
III.
240°C
200°C

tangents coincide at the points of junction, though the *curvature* is necessarily not continuous from one to the other. Hence, as at least a fair approximation to the electro-motive force in terms of difference of temperature in the junctions, we may assume a parabolic function, which up to a certain temperature belongs to one parabola, then changes to another without discontinuity of direction, and so on.

Hence either the iron, or the hard platinum and the platinum-iridium alloys, will be (approximately, at least) represented on my form of Thomson's thermo-electric diagram (*anté* p. 601) by *broken* lines, of which the successive parts are straight. This, contrasted with the (at least nearly) straight lines for pure metals, seems to show that some bodies take successively different states (*i.e.*, become *different substances*) at certain "critical" temperatures, retaining their thermo-electric properties nearly unchanged from one of those critical points to another.

The curve marked IV. in the figure was obtained by plotting against each other the simultaneous indications of the alloy of curve III. and iron, and of the alloy of curve II. and iron, so as to avoid any disturbance from possible peculiarities of palladium. Then, to obtain an idea of the share taken by iron in the results, it was found that the electro-motive force in a circuit formed by the two alloys, or by either with hard Pt, is (for a very great range of temperature) sensibly proportional to the temperature difference of the junctions.

The same result is easily seen from the plate, if we notice that the difference of corresponding ordinates in any two of curves I., II., III., is nearly proportional to the corresponding abscissa. Now, it seems a less harsh supposition that the lines representing platinum and its alloys are nearly straight and parallel, while that of iron is a broken line, than that the latter should be straight and the former all broken at the same temperatures. On the other hand, this latter hypothesis would make k alternately negative and positive in iron, while the former would only require the platinum metals to have values of k alternately less and more negative than that of iron.

I may add that none of the above-mentioned effects can be due to altered electric resistance of the heated junctions, because the galvanometer resistance was about 23 B. A. units, while that of the

iron and platinum wires together was in each case not more than one such unit. The palladium-iron circuit was so much more powerful than the others that a resistance coil of about 146 B. A. units had to be inserted in its course.

Assuming, for a moment, that, as above suggested as at least approximately true, in one of the wires we have $\sigma = k_1 t$ up to the temperature t_1, $\sigma = k_2 t$ up to temperature t_2, &c., we have by the two equations of thermo-dynamics—

$$E = J\left(\Sigma\Pi + \Sigma_1^n \int_{t_r}^{t_{r+1}} \sigma_r dt\right)$$

$$0 = \Sigma\frac{\Pi}{t} + \Sigma_1^n \int_{t_r}^{t_{r+1}} \frac{\sigma_r}{t} dt.$$

Now, if both junctions be under t_1, and if $\sigma = kt$ for the other wire,

$$\delta E = J(\delta\Pi + \overline{k_1 - k}t\delta t)$$

$$0 = \delta\frac{\Pi}{t} + (k_1 - k)\delta t,$$

and we have as before, t_0 being temperature of cold junction,

$$\frac{\Pi}{t} = -(k_1 - k)(T - t)$$

$$E = -(k_1 - k)(t - t_0)\left(T - \frac{t + t_0}{2}\right).$$

But from t_1 to t_2 we have

$$\frac{\Pi}{t} = -(k_2 - k)(T_1 - t)$$

$$E = C - (k_2 - k)(t - t_0)\left(T_1 - \frac{t + t_0}{2}\right).$$

Now, at $t = t_1$ these formulæ must agree, so that

$$(k_2 - k)(T_1 - t_1) = (k_1 - k)(T - t_1)$$

$$C = (t_1 - t_0)\left\{(k_2 - k)T_1 - (k_1 - k)T - (k_2 - k_1)\frac{t_1 + t_0}{2}\right\},$$

whence

$$T_1 = \frac{(k_2 - k_1)t_1 + (k_1 - k)T}{k_2 - k},$$

and

$$C = (t_1 - t_0)(k_2 - k_1)\left(t_1 - \frac{t_1 + t_0}{2}\right) = \frac{1}{2}(k_2 - k_1)(t_1 - t_0)^2.$$

I reserve farther developments of this subject until I have made a sufficient number of experiments with *both* junctions at high temperatures, particularly when these are two of the series of neutral points; and especially until I manage to settle, by one at least of several processes which have occurred to me, whether the multiple neutral points depend upon peculiarities in the behaviour of the iron, or of the platinum, or of both.

[*Added during printing.*—I have since made out that the lines of the diagram are approximately straight, and parallel to the lead line, for the platinum metals, that of hard platinum being below the lead line, while those of most of the other alloys are above it, and that the multiple neutral points depend upon the peculiar sinuosity of the line for iron. I have also obtained curious results of a somewhat similar kind with steel wire. The method I employed was to explore the part of the thermo-electric diagram included between the lines of gold and palladium, by making a multiple arc of these two metals, and varying the ratio of their separate resistances. But I reserve details until I have carefully examined the behaviour of nearly pure iron.]

2. On a Method of Exhibiting the Sympathy of Pendulums.

While making some magnetic experiments lately with Mr Fox Talbot, I happened to notice that two equal rectangular pieces of tin plate, when standing nearly parallel to one another on the pole of a large electromagnet, acted on one another so that a vibration communicated to either was in a few seconds handed over to the other, and *vice versâ*.

The definiteness of the result led me to try the experiment with ordinary bar magnets. Taking two large magnetised bars of almost exactly equal mass, I suspended them with their axes in the same horizontal line, so that their (small) vibrations were executed in that line, their undisturbed periods being very nearly equal, and the distance between them (when at rest) so small compared with their lengths, that we need consider only the magnetic action of the two poles nearest together. With this apparatus the transfer of energy from one pendulum to the other is most beautifully exhibited, for if one only be in motion at starting, the magnets

alternately come sharply to rest at successive equal intervals of time. This arrangement makes an excellent and instructive class experiment, and its value may be greatly increased by placing round the exterior end of one of the magnets a vertical coil of copper-wire connected with a distant galvanometer. The nature of the motion of this magnet at any instant is readily deciphered from the signals given by the reflected light on the galvanometer scale, which is also visible to the whole class. A more complex, but with practice easily intelligible, signal is given by placing the coil round the contiguous ends of the magnets.

The extension of this arrangement to three, four, and more equal magnets, all vibrating in one line, and of nearly equal mass, magnetic power, and (independent) period is of course obvious, and forms a beautiful mechanical illustration of the solution of a differential equation.

In thinking how most simply to explain such results to an elementary class, I was led to the following, which can hardly be new, though I have never met with it, but which is certainly not as well known as it ought to be. Take first the case of the two equal magnets.

Since there are but two moving parts of the system, and each has but *one* degree of freedom, it is obvious that if we can find *two* different forms of motion of the system which, once established, will persist for ever, any motion whatever of the system must be a mere superposition of these two modes with arbitrary amplitudes and epochs. Now, one such mode is obviously the motion of the pendulums *as one piece* at their equilibrium distance from one another. As the magnetic force does not vary during this motion, the time of vibration is that of either pendulum when left to itself. The other fundamental mode is that in which the centre of inertia of the two remains fixed, *i.e.*, the simultaneous displacements of the two magnets are equal and in opposite directions. The time of small oscillations now will evidently be the same as if one of the magnets were held fixed and its magnetic strength doubled. It will, therefore, be shorter or longer than the former period, according as the poles presented to one another attract or repel, and its actual value is easily calculated. Hence, as these small motions separately can be represented by expressions such as $\cos(mt + \epsilon)$,

$\cos(m't + \epsilon')$; the period of any complex vibration is $\frac{2\pi}{m - m'}$, and therefore at intervals of $\frac{\pi}{m - m}$ the configuration of the magnets will be the same to a spectator who changes the side from which he regards them in successive such intervals. Thus, if one magnet was originally at rest, the two will alternately be reduced to rest.

When there are three equal magnets, it is easy to see that one fundamental mode is a swing of the whole as one piece, a second (if we suppose like or unlike poles adjacent to each other at each gap) is the middle magnet and the centre of inertia of the other two fixed, and the third has also the centre of inertia fixed, but the two extreme magnets are at each instant equally deflected in the same direction, while the middle one has a double deflection to the opposite side. It is troublesome, but not difficult, to think out the fundamental modes for four and even for five magnets; but it would be a waste of time to try it in that way for more.

Generally if x_r denote the displacement at time t of the rth magnet, and if we assume the masses, magnetisation, and gaps to be equal, we have

$$\ddot{x}_r + n^2 x_r = \mu\left(\frac{1}{(a + x_r - x_{r-1})^2} - \frac{1}{(a + x_{r+1} - x_r)^2}\right)$$

$$= \frac{\mu}{a^3}(x_{r-1} + x_{r+1} - 2x_r),$$

except for the ends of the series where $r = 1$, and $r = m$, the number of magnets.

Hence, multiplying by λ_r and adding, we have

$$\ddot{\xi} + p^2\xi = 0,$$

where

$$\xi = \Sigma\lambda_r x_r$$

$$p^2 = \frac{\lambda_1\left(n^2 + \frac{\mu}{a^3}\right) - \lambda_2\frac{\mu}{a^3}}{\lambda_1} = \frac{-\lambda_1\frac{\mu}{a^3} + \lambda_2\left(n^2 + \frac{2\mu}{a^3}\right) - \lambda_3\frac{\mu}{a^3}}{\lambda_2} = \&c.$$

It will be sufficient to work this out for three magnets. Here, if we put $\frac{\mu}{n^2a^3} = e$, we have

$$\frac{p^2}{n^2} = 1 + e - e\frac{\lambda_2}{\lambda_1} = -e\frac{\lambda_1}{\lambda_2} + 1 + 2e - \frac{\lambda_3}{\lambda_2}e = -\frac{\lambda_2}{\lambda_3}e + 1 + e.$$

$$\therefore\ \frac{\lambda_2}{\lambda_1} = \frac{\lambda_2}{\lambda_3},\ \text{or } \lambda_1 = \lambda_3,\ \text{besides } \lambda_2 = 0;$$

whence

$$-\frac{\lambda_2}{\lambda_1}e = -e\frac{\lambda_1}{\lambda_2} + e - \frac{\lambda_1}{\lambda_2}e$$

$$\left(\frac{\lambda_2}{\lambda_1}\right)^2 + \frac{\lambda_2}{\lambda_1} - 2 = 0\,,$$

$$i.e.,\ \frac{\lambda_2}{\lambda_1} = 1,\ \text{or } -2,\ \text{or } 0.$$

Thus $p^2 = n^2$, or $n^2(1 + 3e)$, or $n^2(1 + e)$. There is no farther difficulty in applying the method to magnets of different masses or magnetic strengths; but it is interesting to observe that, by properly adjusting the gaps in terms of the masses and magnetisation of the bars, any set of magnets whatever can be brought to behave (for small oscillations) as if they were in all respects equal to each other and arranged at equal distances.

When there is an infinite series of magnets arranged in this way the equation above may be written

$$\left[\left(\frac{d}{dt}\right)^2 + n^2 + \frac{\mu}{a^3}\frac{(\mathrm{D} - 1)^2}{\mathrm{D}}\right]x_r = 0\,,$$

where

$$\mathrm{D}x_r = x_{r+1}\,,$$

of which the general integral is easily found.

When the number of magnets (m) is finite, and they are arranged in a closed curve, we have the conditional equation

$$(\mathrm{D}^m - 1)\,x_r = 0\,.$$

In this case the general solution may be elegantly expressed in terms of the m^{th} roots of unity. It leads to some curious properties of determinants, whose development will form an excellent exercise for the student. Thus, writing in succession $1, 2, \ldots, m$ for r; and putting

$$l = \frac{a^3}{\mu}\left(\left(\frac{d}{dt}\right)^2 + n^2\right),$$

the first of the above equations gives, by the help of the second, after the elimination of the displacements

$$\begin{vmatrix} l-2 & 1 & & & & & 1 \\ 1 & l-2 & 1 & & & & \\ & 1 & l-2 & 1 & & & \\ & & & \&c. & & & \\ & & & & 1 & l-2 & 1 \\ 1 & & & & & 1 & l-2 \end{vmatrix} = 0.$$

This is a particular case of the determinant,

$$\begin{vmatrix} p & q & r & s & \dots\dots \\ z & p & q & r & \dots\dots \\ y & z & p & q & \dots\dots \\ \dots & \dots & \dots & \dots & \dots \\ \dots\dots & \dots & z & p & q \\ \dots\dots & \dots & y & z & p \end{vmatrix}$$

which, equated to zero, gives the result of elimination of θ between the equations

$$p + q\theta + r\theta^2 + \dots\dots\dots + z\theta^{m-1} = 0,$$

$$\theta^m - 1 = 0.$$

Its factors are obviously to be found by substituting in succession the several m^{th} roots of unity in the expression

$$p + q\theta + \dots\dots\dots + z\theta^{m-1}.$$

The form of its minors, on which depends the solution of the pendulum question, follows easily from these properties; and from them we in turn easily obtain the value of the same determinant when bordered, as it will be in the pendulum case if the series of magnets be finite and *not* closed. The question forms a very interesting illustration of the linear propagation of disturbances in a medium consisting of discrete, massive, particles—when only contiguous ones act on one another. For, if we put

$$D = \epsilon^{a\frac{d}{dy}},$$

and alter the value of μ, we have by taking a small,

$$\left[\left(\frac{d}{dt}\right)^2 + n^2 + \mu\left(\frac{d}{dy}\right)^2\right] x = 0;$$

which, with $n = 0$, is the usual equation for sound, provided the particles repel one another. Of course we can easily extend the investigation so as to include the more complex cases where the mutual actions of all the poles are taken into account. The result is not altered in form; but it might be curious to inquire whether the retention of n^2 in the equation might not give some hints as to the formation of a dynamical hypothesis of the action of transparent solids on the luminiferous ether. This, however, I cannot enter upon at present.

4. On Some Quaternion Integrals. Part II. By Professor Tait.

(*Abstract.*)

Commencing afresh with the fundamental integral

$$\iiint S . \nabla\sigma ds = \iint S . U\nu\sigma ds ,$$

put

$$\sigma = u\beta$$

and we have

$$\iiint (S . \beta\nabla) u ds = \iint u S . \beta U\nu \, ds ;$$

from which at once

$$\iiint \nabla u \, ds = \iint u U\nu \, ds , \quad . \quad . \quad . \ (a),$$

or

$$\iiint \nabla\tau \, ds = \iint U\nu . \tau ds . \quad . \quad . \quad . \ (b).$$

Putting $u_1\tau$ for τ, and taking the scalar, we have

$$\iiint (S(\tau\nabla) . u_1 + u_1 S . \nabla\tau) \, ds = \iint u_1 S . U\nu\tau \, ds$$

whence

$$\iiint (S(\tau\nabla)\sigma + \sigma S . \nabla\tau) \, ds = \iint \sigma S . U\nu\tau \, ds \quad . \quad . \ (c).$$

As one example of the important results derived from these simple formulæ, I take in this abstract the following, viz. :—

$$\iint V . (V . \sigma U\nu) \tau ds = \iint \sigma S . U\nu\tau \, ds - \iint U\nu S . \sigma\tau ds ,$$

where by (c) and (a) we see that the right hand member may be written

$$= \iiint (S.(\tau\nabla)\sigma + \sigma S.\nabla\tau - \nabla S.\sigma\tau)\, d\varsigma$$
$$= -\iiint V.V(\nabla\sigma)\tau d\varsigma \quad . \quad . \quad . \quad . \quad . \quad (d).$$

This, and similar formulæ, are applied in the paper to find the potential and vector-force due to various distributions of magnetism. To show how this is introduced, I briefly sketch the mode of expressing the potential of a distribution.

Let σ be the vector expressing the direction and intensity of magnetisation, per unit of volume, at the element $d\varsigma$. Then if the magnet be placed in a field of magnetic force whose potential is u, we have for its potential energy

$$E = -\iiint S(\sigma\nabla)u d\varsigma$$
$$= \iiint u S(\nabla\sigma) d\varsigma - \iint u S.U\nu\sigma ds.$$

This shows at once that the magnetism may be resolved into a volume-density $S(\nabla\sigma)$, and a surface-density $-S.U\nu\sigma$. Hence, for a solenoidal distribution,

$$S.\nabla\sigma = 0.$$

What Thomson has called a lamellar distribution (*Phil. Trans.* 1852), obviously requires that

$$S.\sigma d\rho$$

be integrable without a factor; *i.e.*, that

$$V.\nabla\sigma = 0.$$

A complex lamellar distribution requires that the same expression be integrable by the aid of a factor. If this be u, we have at once

$$V.\nabla(u\sigma) = 0,$$

or

$$S.\sigma\nabla\sigma = 0.$$

With these preliminaries we see at once that (d) may be written

$$\iint \mathrm{V}.(\mathrm{V}.\sigma\mathrm{U}\nu)\tau ds = -\iiint \mathrm{V}.\tau\mathrm{V}.\nabla\sigma d\varsigma - \iiint \mathrm{V}.\sigma\nabla\tau d\varsigma + \iiint \mathrm{S}\alpha\nabla.\tau d\varsigma.$$

Now, if $\tau = \nabla\left(\frac{1}{r}\right)$, where r is the distance between any external point and the element $d\varsigma$, the last term on the right is the vector-force exerted by the magnet on a unit pole placed at the point. The second term on the right vanishes by Laplace's equation, and the first vanishes as above if the distribution of magnetism be lamellar, thus giving Thomson's result in the form of a surface integral.

Another of the applications made is to Ampère's *Directrice de l'action électrodynamique*, which (*Quarterly Math. Journal*, Jan. 1860) is the vector-integral

$$\int \frac{\mathrm{V}\rho d\rho}{\mathrm{T}\rho^3},$$

where $d\rho$ is an element of a closed circuit, and the integration extends round the circuit. This leads again to the consideration of relations between single and double integrals.

[Here it may be well to note that, by inadvertence, I wrote σ for τ towards the end of the abstract of the former part of this paper, thus giving the result a false generalisation depending on the fact that τ had been made subject to the condition

$$\mathrm{S}.\nabla\tau = 0,$$

while no such restriction was imposed on σ. With this restriction most of the results already given (*Proc. ante* p. 320) are correct, but the general forms in the paper itself are as follows, being deducible at once from the first expression in the abstract:—

$$\iint \mathrm{S}.\mathrm{U}\nu\nabla^2\sigma ds - \iint \mathrm{S}.\mathrm{U}\nu\nabla\mathrm{S}.\nabla\sigma ds = \int \mathrm{S}.\nabla\sigma d\rho,$$

and

$$\iint \mathrm{U}\nu\nabla^2\mathrm{P} ds - \iint \mathrm{S}.\mathrm{U}\nu\nabla.\nabla\mathrm{P} ds = \int \mathrm{V}(d\rho\nabla)\mathrm{P};$$

giving finally

$$\iint \mathrm{V}.\mathrm{U}\nu\nabla^2\sigma ds - \iint \mathrm{S}.\mathrm{U}\nu\nabla.\mathrm{V}\nabla\sigma ds = \int \mathrm{V}.\mathrm{V}(d\rho\nabla)\sigma.]$$

Returning to the electrodynamic integral, note that it may be written

$$-\int V \, . \, (d\rho \nabla) \frac{1}{r} \, ,$$

so that, by the corrected formula just quoted, its value as a surface integral is

$$\iint S \, . \, U\nu\nabla \, . \, \nabla \frac{1}{r} \, ds - \iint U\nu\nabla^2 \frac{1}{r} \, ds.$$

Of this the last term vanishes, unless the origin is in, or infinitely near to, the surface over which the double integration extends. The value of the first term is seen (by what precedes) to be the vector-force due to uniform normal magnetisation of the same surface.

Also, since

$$\nabla U\rho = -\frac{2}{T\rho} \, ,$$

we obtain at once

$$-2\iiint \frac{d\varsigma}{T\rho} = \iint S \, . \, U\rho U\nu ds \, ,$$

whence, by differentiation, or by putting $\rho + \alpha$ for ρ, and expanding in ascending powers of $T\alpha$ (both of which tacitly assume that the origin is external to the space integrated through, *i.e.*, that $T\rho$ nowhere vanishes), we have

$$-2\iiint \frac{d\varsigma U\rho}{T\rho^2} = \iint \frac{V \, . \, U\rho V \, . \, U\nu U\rho}{T\rho} \, ds = 2\iint \frac{U\nu ds}{T\rho} \, ;$$

and this, again, involves

$$\iint \frac{U\nu ds}{T\rho} = \iint \frac{U\rho}{T\rho} S \, . \, U\nu U\rho ds \, .$$

The interpretation of these, and of more complex formulæ of a similar kind, leads to many curious theorems in attraction and in potentials. Thus, from (a) we have

$$\iiint \frac{\nabla t}{T\rho} d\varsigma - \iiint \frac{tU\rho}{T\rho^2} d\varsigma = \iint \frac{tU\nu}{T\rho} ds \, ,$$

which gives the attraction of a mass of density t in terms of the potentials of volume distributions and surface distributions. Putting

$$\sigma = it + jt_2 + kt_3,$$

this becomes

$$\iiint \frac{\nabla\sigma ds}{T\rho} - \iiint \frac{U\rho \,.\, \sigma ds}{T\rho^2} = \iint \frac{U\nu \,.\, \sigma ds}{T\rho}.$$

By putting $\sigma = \rho$, and taking the scalar, we recover a formula given above; and by taking the vector we have

$$V\iint U\nu U\rho ds = 0.$$

This may be easily verified from the formula

$$\int P d\rho = V\iint U\nu \,.\, \nabla P ds,$$

by remembering that

$$\nabla T\rho = U\rho.$$

Again if, in the fundamental integral, we put

$$\sigma = tU\rho,$$

we have

$$\iiint \frac{S(\rho\nabla)t}{T\rho} ds - 2\iiint \frac{tds}{T\rho} = \iint tS \,.\, U\nu U\rho ds.$$

5. On the Currents produced by Contact of Wires of the same Metal at different Temperatures. By W. Durham, Esq. Communicated by Professor Tait.

At the suggestion of Professor Tait, I undertook the investigation of the momentary thermo-electric current developed when two conductors or wires of the *same* metal are brought into contact, the one being at a different temperature from the other.

Platinum was chosen as the most suitable metal to experiment with, in the first instance, as it is free from the interfering action of oxidation at high temperatures.

The following arrangement of apparatus was employed:—

1. A long iron bar, one of those used by the late Principal Forbes in his experiments on the conduction of heat, was heated at one end in the usual manner. This formed the source of heat at once steady and graduated, so that, by contact with it at various parts, the platinum wire experimented with could be kept at any required temperature.

2. Small glass tubes were fitted into holes in the bar at regular intervals, and turned over a little at the edge in the form of a lip. These served the double purpose of preventing metallic contact with the bar (and thus introducing ordinary thermo-electric currents), and also served as guides to the same point of contact in each experiment.

3. A small iron bar kept at the temperature of the room.

4. A reflecting galvanometer (with somewhat massive mirror and magnet, so as to "integrate"), with a scale placed at the distance of six feet, so that the smallest deflection of the needle could be readily observed and measured.

5. Two pieces of the same platinum wire connected with the galvanometer in the usual manner.

The mode of working was as follows:—The free end of one of the platinum wires rested on the small bar, and was thus kept at the temperature of the room. The free end of the other wire was placed in one of the glass tubes on the heated bar, and, while in that position, and after it had attained the temperature of the bar at that particular spot, the wire from the small bar was brought into contact with it, and the sudden deflection of the galvanometer needle noted.

With this arrangement very good and steady results were obtained when care was taken to keep the wires perfectly clean, and to apply the same amount of pressure in making contact in every experiment, because any deficiency of contact increased the resistance so as greatly to affect the currents.

The results show that for platinum wire the current, as indicated by the deflection of the galvanometer needle, is exactly as the difference of temperature between the two wires.

To show the steadiness of the results, I give the details of one experiment—

	Temperature of Hole.	Difference of Temperature.	Galvanometer Deflection.	Mean.
No. 1.	325°C.?	310° ?	215, 220, 225, 220, 225, 235, 240, 230, 240, 240, 237, 245, 235, 220, 250, 230, . .	= 231·7
2.	208°	193°	140, 140, 135, 130, 142, 130, 130, 130, 132, 128, 132, 130, 130, 135, 130, 132, 135, 140, 140, 140, 130, 135, 135, . .	= 134·
3.	144°	129°	90, 90, 90, 92, 90, 85, 85, 90, 85, 87, 85, 85, 90, 85, 80, 80, 90, 85, 90, 90, . . .	= 85·
4.	103°	88°	62, 60, 60, 60, 55, 60, 55, 60, 60, 60, 60, . . .	= 59·27
5.	78°	63°	42, 42, 44, 44, 44, 40, 50, 47, 50, 47, 50, . . .	= 45·5
6.	56°	41°	38, 35, 32, 30, 30, 32, 35, 35, 33, 35, 35, 35, 35, 35, 35, 38, 38, 35, 35, 38, . . .	= 34·7

The following are the means of a great number of experiments, the mean values of the current being all multiplied by a common factor :—

No. 1.		No. 2.		No. 3.	
Difference of Temperature in Degrees Cent.	Current.	Difference of Temperature in Degrees Cent.	Current.	Difference of Temperature in Degrees Cent.	Current.
21°	19·	50°	55·5	9°	9·6
30°	30·	53°	54·5	14°	13·
42°	38·3	63°	68·	20°	19·
60°	59·	68°	70·	28°	26·
88°	89·	74°	73·	39°	34·
92°	90·	88°	89·	61°	65·
134°	132·5	105°	101·	84°	76·
136°	135·	109°	105·	124°	120·
139°	138·	129°	127·	131°	120·?
140°	142·	152°	120·?	196°	192·
		167°	161·5	?	314·
		193°	201·		
		?	266·		
		?	347·		

With the same apparatus as in the foregoing, I next tried heating *both* wires considerably above the temperature of the room,

till, however, keeping one wire at a higher temperature than the other. The result in this case was as in the former. The current was exactly as the difference of temperature. The following are the means of the experiment:—

Temperatures in Degrees Cent.	Current.
203° — 142° = 61°	64·5
142° — 100° = 42°	48·
100° — 76° = 24°	30·

With more sensitive galvanometer,—

320° ? — 205° = 115° ?	120· *
205° — 143° = 62°	64·5
143° — 102° = 41°	42·
102° — 76° = 26°	28·5

6. Remarks on the Deep-Water Temperature of Lochs Lomond, Katrine, and Tay. By Alexander Buchan.

In the communications made by Sir Robert Christison to the Society in December and April last on the deep-water temperature of Loch Lomond, from observations made by him with a Miller-Casilla thermometer, these important facts were stated:—

(1.) On 12th October 1871, the temperature at the surface was 52·°0, from which it fell, on descending, till at 300 feet below the surface it stood at 42°·0, and this temperature of 42°·0 was uniformly maintained at greater depths or to 518 feet, the depth of the loch at the place of observation.

(2). On 18th November following, the surface temperature was 46°·0; at depth of 250 feet, 42°·25; at 270 feet and lower depths, 42°·0.

(3.) On the 10th April 1872, the temperature at the surface was 43°·0; at 150 feet, 42°·1; and from 200 to 594 feet, 42°·0.

Hence it appears that there is a stratum of water of considerable thickness at the bottom of this loch of uniform temperature; that the upper surface of this stratum of deep water of uniform temperature was about 100 higher on the 10th of April than it was in the

* Results varied considerably owing to working so near the flame—varying from 104° to 126°·

beginning of winter, or on the 18th November; and that this deep water temperature probably remains constantly at, or very near, 42°·0.

Sir Robert asked me for a statement of the temperature of the air at Loch Lomond from 18th November 1871 to 10th April 1872, or during the time that the cold stratum of water of the uniform temperature of 42°·0 had increased about 100 feet in thickness. This I have prepared from the observations made at Balloch Castle, by Mr David Hill, the observer of the Scottish Meteorological Society at that place. Balloch Castle is at the foot of the loch, and 72 feet above its surface. The table showed the mean temperature of each day during the time,—the mean of the maximum and minimum temperatures of each day being assumed as the mean temperature of that day. Of this table an abstract is given below, from which it appears that the mean temperature, from

November	18	to	30	was	38°·0,	or	2°·5 below	the average,
December	1	„	31	„	39°·4,	„	0°·4 „	„
January	1	„	31	„	40°·8,	„	2°·3 above	„
February	1	„	29	„	43°·3,	„	3°·3 „	„
March	1	„	31	„	43°·6,	„	2°·1 „	„
April	1	„	10	„	45°·6,	„	1°·4 „	„

The average temperature of the 145 days was 41·7, which 1°·4 above the average of past years.

Tåking the observed mean temperature of each day for Edinburgh as calculated by the late Principal Forbes,* and applying to these the differences observed between Balloch Castle and Edinburgh, the normal temperature of each day at Balloch Castle was calculated. In this way the divergence of the temperature of each of the 145 days from its normal was ascertained. The amount for each day was given in a table,—temperatures above the average being given in red ink, under the average in blue. An abstract of this table is given below, from which it appears that there were four cold, and four mild periods, as under :—

* Trans. of the Society, vol. xxii. p. 351.

Cold Periods.

November 18	to	December 10,	or	23	days,	4°·6	under average,
December 20	„	„ 23,	„	4	„	3°·9	„
January 5	„	January 10,	„	6	„	1°·0	„
March 20	„	April 6,	„	18	„	3°·0	„
		Average,		51	days,	3°·4	„

Mild Periods.

December 11	to	December 19,	or	9	days,	4°·1	above average,
„ 24	„	January 4,	„	12	„	3°·5	„
January 11	„	March 19,	„	69	„	3°·9	„
April 7	„	April 10,	„	4	„	6°·0	„
		Average,		94	days,	4°·0	„

Hence during this period the temperature was under the average of the season on 51 days, the deficiency amounting to a mean of 3°·4; and above the average on 94 days, the excess amounting to a mean of 4°·0. The most markedly mild period extended over 69 days, viz., from 11th January to 19th March, during which the temperature was on an average of 3°·9 above that of the season; and as already stated, the temperature was, for the whole period of 145 days, 1°·4 above the average.

It may be concluded that in ordinary winters the stratum of water of uniform temperature will be thicker than Sir Robert Christison found it to be this year in the beginning of spring; in other words, that it will be nearer the surface than 170 feet.

In the end of last week, Mr James Leslie, C.E., kindly sent me some highly interesting and valuable observations on the deep-water temperature of Lochs Tay, Katrine, and Lomond, made by the late Mr James Jardine, C.E., in 1812 and 1814. These I have now very great pleasure in laying before the Society. They were taken in fathoms, and the temperature in degrees centigrade which are here reduced to Eng. feet, and degrees Fah.

* The general results of these observations were given by Sir John Leslie in his "Treatises on Various Subjects of Natural and Chemical Philosophy," Edinburgh 1838, p. 281.

Observations of the Deep-Water Temperature of Lochs Tay, Katrine, and Lomond, by the late James Jardine, Esq., C.E.

Depth.	Loch Tay. Aug. 12, 1812.	Loch Katrine. Sept. 3, 1814.	Loch Katrine. Sept. 7, 1812.	Loch Lomond Sept. 8, 1812.
Surface	57°·2	56°·8	57°·9	59°·5
30 feet	...	56°·7	...	...
60 "	...	49°·6	50°·9	...
90 "	...	45°·5	...	44°·1
120 "	...	44°·4	43°·5	...
150 "	...	43°·3	...	...
180 "	...	42°·3	...	...
210 "	43°·2	...	41°·5	...
240 "	...	...	...	41°·7
300 "	...	...	41°·5	...
360 "	...	...	41°·5	...
420 "	41°·9	...	...	...
480 "	...	41°·7	41°·4	41°·7
540 "	...	...	...	41°·5
600 "	...	...	...	41°·5

These results are strikingly accordant with those obtained by Sir Robert Christison, The difference as regards the deep-water temperature of Loch Lomond may be, and probably is, only instrumental.

These observations were made in the summer and early autumn, or when the temperature of the sea and of lakes is about the annual maximum. Taken in connection with Sir Robert's observations, they warrant the conclusion that the deep-water temperature of Loch Lomond remains during the whole year either absolutely at, or very nearly at, the low figure of 42°·0.

The observations also show that this is not a peculiarity of Loch Lomond, but that it is also a characteristic of Lochs Katrine and Tay, and most probably of other deep waters.

The mean annual temperature of the air at Loch Lomond, from the mean at Balloch Castle, calculated on the 13 years' average, ending 1869, is 48°·0,* which is 6°·0 higher than the uniform deep-water temperature of the loch. The deep-water temperature

* In this and following temperatures 0°·2 has been added, in order to bring them to the level of the loch, which is 72 feet lower than the thermometers at Balloch Castle.

is, therefore, not determined by the mean annual temperature of air over this part of the earth's surface.

From Forbes' "Climate of Edinburgh," it is seen that the temperature there is under the annual mean from the 21st October to the 26th April. Assuming that this holds good for Balloch Castle, then the mean temperature for the cold half of the year will be, from—

October	21 to 31, . . .	46°·0
November	1 to 30, . . .	41°·7
December	1 to 31, . . .	40°·9
January	1 to 31, . . .	38°·6
February	1 to 28, . . .	39°·8
March	1 to 31, . . .	40°·5
April	1 to 26, . . .	45°·8

The mean of these 188 days is therefore 41°·4.

The close approximation of this temperature of 41°·4 to 42°·0, the deep-water temperature of the loch, is such as to suggest *that it is the mean temperature of the cold half of the year which determines the temperature of the lowest stratum of water at the bottom of deep lakes*, so long as the deep-water temperature does not fall below that of the maximum density of the water. As this principle, if established, would be of great importance in many questions of physical research, such as the deep-water temperature of the Mediterranean Sea, which Dr Carpenter has very accurately ascertained, in its connection with the larger question of general oceanic circulation, it well deserves further investigation.

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